Knowledge.

With which is incorporated Hardwicke's Science Gossip, and the Illustrated Scientific News.

A Monthly Record of Science.

Conducted by Wilfred Mark Webb, F.L.S., and E. S. Grew, M.A.

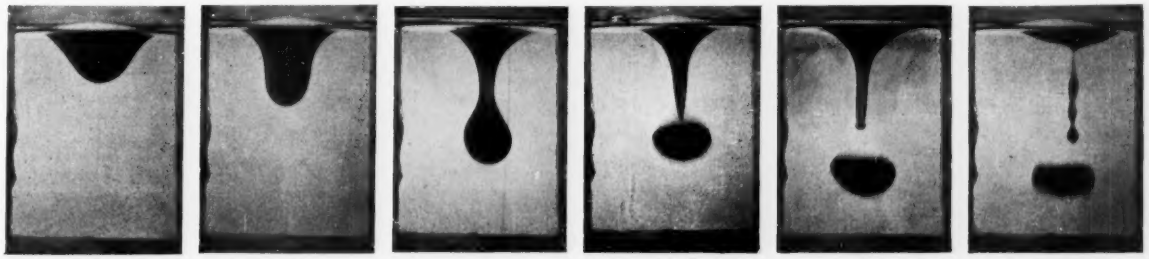
JANUARY, 1911.

A SIMPLE EXPERIMENT ON THE FORMATION OF DROPS OF LIQUID.

By CHARLES R. DARLING, A.R.C.Sc.I., F.I.C.

WHEN a suspended mass of liquid attains a definite size it becomes too heavy to be sustained by the adhesion of the liquid to the surface from which it hangs, and, consequently, a portion breaks away, resulting in the formation of a spherical drop. When the liquid hangs in air, as in the case of water on the end of a leaky tap, the drop breaks away too quickly to permit the process of separation to be observed by the unaided eye. In order to study the changes in outline undergone by the suspended liquid, instantaneous photography and rapidly intermittent light have been employed, and have succeeded in disclosing the beautiful transition shapes which accompany the detachment of the drop. Either of these methods of observation, however, demands the use of elaborate instruments, and it is here proposed to describe an experiment of the simplest description, which not only enables the process to be followed easily by the eye, but is unique in the respect that the formation of the drops is automatic and continuous. It was arrived at by the author as the result of a comparative study of the physical properties of water and other liquids of approximately equal density.

A glass beaker, about six inches high and four inches in diameter, is filled with water to the height of about four and a-half inches, and seventy or eighty cubic centimetres of commercial aniline are added, which will sink to the bottom of the vessel. The temperature of the beaker and its contents is now raised to 75° or 80° C. by means of a burner, when it will be observed that the aniline will rise to the surface of the water, from which it will hang in a mass of curved outline. Almost immediately the suspended aniline commences to alter in shape; and gradually a large drop, an inch or more in diameter, detaches itself from the mass and falls through the water. The formation of this drop takes place so slowly, owing to the aniline being buoyed up by the water beneath, that all the changes of shape associated with the process may be observed distinctly. The appearance presented is well shown in the accompanying photographs, taken by Mr. B. Abel, of the City and Guilds Technical College, Finsbury, with an ordinary hand camera. In No. 1 the formation is commencing; No. 2 represents the stage just preceding the formation of the neck; in No. 3 the neck has formed, and in No. 4 the drop has just broken away; No. 5 shows the flattening of the drop due to the shock of breakage,



1 2 3 4 5 6

The formation of a drop of aniline.

and also shows the neck to have taken a cylindrical form ; whilst in No. 6 the distortion of the drop has proceeded further, and the cylindrical neck, being unstable, is seen to be breaking up into three separate portions, each of which forms a sphere. These photographs, however, do not give such a complete idea of the process as may be obtained by watching the experiment, as only six detached stages are represented. Variations occur, also, in the fate of the neck, which sometimes shrinks back into the mass remaining on the surface; sometimes breaks off and forms a single sphere, and often several spheres of varying sizes. The exact method of taking the photographs will be explained later.

And now, the detached drop having fallen to the bottom of the beaker, comes the surprising part of the experiment. The fallen drop is seen gradually to rise to the surface, where it joins the mass from which it previously broke away. At once another drop commences to form, and having become detached, falls and rises in the same manner as the previous drop. So long as the temperature of the water is maintained at 70° C or over, this procedure continues indefinitely. The forces at work on the drop perform a Sisyphean task.

Here it may be explained that the photographs shown do not represent one and the same drop, but that a snap-shot was taken at different phases in the formation of six separate drops. The uniformity with which the process is repeated is thus exhibited in a striking manner, as the photographs might easily be taken to represent six stages in the formation of a single drop. It may be added that the temperature should never exceed 85° C, nor fall below 70° C if photographs are being taken ; and that the distorting effect of the glass beaker may be overcome by placing it in a rectangular vessel with glass sides, also containing hot water. This plan was followed by Mr. Abel in securing the photographs shown ; it is also suitable for the optical projection of the experiment.

The explanation of the formation of the drops and their subsequent ascent to the surface is, in the main, simple. Aniline is a liquid which at low temperatures is denser than cold water, but at high temperatures lighter than hot water, owing to its higher degree of expansion. If dropped into water below 60° C, aniline will sink ; but if the temperature of the water exceed 65° C, the aniline, after becoming warmed by its surroundings, will rise. Hence, in the experiment under notice, the aniline ascends to the surface of the water owing to its lesser density. By spreading out on the surface, however, the aniline is cooled more than the water beneath, and soon becomes denser than the water in consequence of this cooling. The aniline then tends to sink, and a drop breaks off as shown in the photographs. By passing through the hot water, however, the temperature of the detached drop rises, and if the temperature exceed 65° the drop will again become lighter than its surroundings and will ascend to the surface. The success of the experiment depends upon this remarkably delicate balance in the temperature-density relations of aniline and water. It must be stated, however, that aniline is partially soluble in water, and hence the drop is falling not through pure water, but a saturated solution of aniline in water. Questions with regard to the surface tensions of the liquids would also have to be entered into in giving a detailed explanation ; and these apparently minor details probably furnish the reason why certain other liquids, which might be expected to behave in the same manner if substituted for aniline in the experiment, fail to do so. So far as investigations have been conducted, no other liquid has been made to operate automatically in producing and reproducing the drop.

One other feature of the experiment is deserving of notice. It may be made to represent the principle of elementary heat engines. The lower part of the water serves as the source of heat ; the atmosphere above the surface acts as the refrigerator to which the rejected heat is given ; and work could be done by the moving drop. It would not be difficult to construct an indicator diagram for this rudimentary heat-engine, based on Carnot's well-known cycle, for the drop passes through a regular and recurrent set of operations. A beaker of hot water and some aniline may thus be made to teach many useful lessons, and furnish a further example of how the profoundest scientific truths may frequently be deduced from the simplest experiments.

THE GREAT NEBULA IN ORION.

THE photograph of the Great Nebula in Orion (reproduced on the opposite page) is made from a negative which we owe to the courtesy of Sir William Christie, F.R.S., the recent Astronomer Royal, and is one of the triumphs of celestial photography at the Royal Observatory, Greenwich. No finer photograph of the nebula is in existence. The reproduction is enlarged three diameters, from a negative taken by Mr. Melotte, on December 1st, 1899, with the thirty-inch reflector, the focal length eleven feet five inches, and the exposure two and a quarter hours. The nebula seen through a telescope glows like a vast filmy cloud of emerald light. Its brightest portions have edges sharp as in an engraving, and are side by side with regions of the most intense blackness.

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The Great Nebula in Orion, from a negative taken December 1st, 1899, with 30-inch Reflector.
Focal length 11-ft. 5-in. Exposure 2¼ hours.

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HOW AND WHY DO LEAVES FALL?

By G. S. BOULGER, F.L.S., F.G.S.

THERE is no element which contributes more to the difference of the landscape as we travel from the Equator northward than the prevalent character of the foliage of the trees. In the tropical jungle the bulk of the trees are dark green, thick-leaved evergreens, a characteristic which extends northward in the more insular moist climates of coast and island regions, notably exemplified by the flora of Japan. In the Cooler Temperate Zone the predominant trees are dicotyledonous angiosperms, the "broad-leaved trees" of our foresters, with smaller, thinner leaves than those of the jungle, lighter in tint, producing a less dense shade, and, for the most part, falling in autumn. Northward of these again the polar limit of arborescent vegetation is reached by the striking Sub-Arctic Zone of "needle-leaved" conifers, mostly evergreen.

It must be noted, in passing, that this term "evergreen," though often true enough of a tree, does not apply to the individual leaf. A tree is evergreen when it retains the leaves of one year at least until after those of the next season are unfolded. We have numerous gradations, from our ordinary "deciduous" species, which are bare of leaves for five or six months in the year, through such cases as that of the privet, which retains its leaves through a mild winter, and that of the holm oak which is only stript by exceptional frost, to such evergreens as the holly, or the cedars and pines, that retain their needles for several successive years.

If we look at the question of leaf-fall no longer geographically, but from the point of view of the systematic botanist, we find that the lower and simpler types of leaves do not fall. The primitive leaves of mosses have no articulation at their base: the elaborately-divided fronds of most tree-ferns wither and hang their dead stalks downwards from the stem: the needles of conifers wither similarly, generally after being several years on the tree; and the simple sheathing leaves of most Monocotyledons have not so perfect a system of articulation as we find in the Dicotyledons, especially those with compound leaves.

A thoughtless, unobservant conclusion would be that the leaf dies and then, and consequently, falls off; but this is far from being the case. Preparations may begin for the fall of the leaf almost as soon as it is formed; and in many cases the leaf is moist and its cells fairly inflated when it falls. As far back as 1758, Duhamel ascribed the fall of the leaf to a layer of tissue between the stem and the leaf, which remained "herbaceous," *i.e.*, capable of growth, but could not stand winter cold; whilst Vrolik, in 1797, spoke of the absorption of a layer between the dead and living parts but belonging itself to the living. In 1848 a Dr. Inman, in a paper communicated to the Literary and Philosophical Society of Liverpool, described an inward extension of the cork of the bark and disruption taking place through cellular tissue external to this corky layer, from without inwards

From a photomicrograph *by F. Noad Clark.*

FIGURE 1.
Longitudinal section of a stem and the base of a leaf stalk of horse chestnut shewing the light coloured cork layer that causes leaf-fall.

> "The provision for the separation," he writes, "being once complete, it requires little to effect it; a desiccation of one side of the leaf-stalk, by causing an effort of torsion, will readily break through the small remains of the fibro-vascular bundles; or the increased size of the coming leaf-bud will snap them; or, if these causes are not in operation, a gust of wind, a heavy shower, or even the simple weight of the lamina, will be enough to disrupt the small connections and send the suicidal member to its grave. Such is the history of the fall of the leaf. We have found that it is not an accidental occurrence, arising simply from the vicissitudes of temperature and the like, but a regular and vital process, which commences with the first formation of the organ, and is completed only when that is no longer useful; and we cannot help admiring the wonderful provision that heals the wound even before it is absolutely made, and affords a covering from atmospheric changes before the part can be subjected to them."

In 1859 Hugo von Mohl, the illustrious founder of the cell theory, chanced to spend his autumn vacation at home, so that he observed the successive fall of the leaflets and the leaf-stalk in the leguminous *Gymnocladus canadensis* with the conveniences of his laboratory at hand. He found that

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a layer of cork already extended through the cellular tissue at the base of the petiole in September. Immediately above this a layer of cells had become brown (suberised); and, separated from this by two or more rows of the ordinary colourless polyhedral cells of the leaf-stalk, what he termed the separating, or "absciss," layer originated. This only formed between the 4th and the 15th of October, extending across the stalk from the inner or axillary surface, and contained in its cells protoplasm and starch-grains. It is, in fact, what we now term "secondary meristem." Von Mohl only recognised two layers of cells in the absciss-layer, which he believed to split apart, while he thought that the fibro-vascular bundles were broken mechanically by the weight of the blade and the strain of wind and rain. He perceived, however, that the fall of the leaflets between the 10th and 20th of October, and the subsequent fall of the petioles was independent of the cork-layer formed at least a month before. This cork layer, in fact, is not formed in advance in those ferns which are deciduous, in beech, elm or most oaks. Von Mohl also noticed that when leaves fell suddenly, after an autumn frost, a thin layer of ice had formed in the delicate sappy cells of the absciss-layer, torn cell-walls evidencing the violence of the disruption.

In 1863, Julius Sachs traced the gradual removal of the contents of the leaf-cells. The protoplasm and nuclei are dissolved, the chlorophyll-granules become disintegrated, the starch disappears, leaving only the few yellow granules, or the reddened cell-sap, which produce our autumn tints; while starch, potash and phosphoric acid travel down the leaf-stalks to be stored up in the twigs, and only the waste or end-products of metabolism, calcium-oxalate crystals, resins and alkaloids remain to be thrown off with the falling leaves.

FIGURE 2.
A beech which has lost the leaves from the ends of the shoots.

FIGURE 3.
The terminal leaves still remaining on linden tree.

In 1882, M.M. Guignard and Van Tieghem returned to the study of *Gymnocladus*; but began their investigation in the middle of June. They found that no cork is formed at the base of the leaflets. It is not worth while to heal the wound on the leaf-stalk which is itself to fall in a day or so. The suberised layer was formed at the base of the main petiole by the middle of June: then a layer of meristem, the "phellogen" or cork-cambium, originates below it and the absciss-layer above it, before the end of June. This layer spreads inwards from the epidermis through the cellular tissue of the bast and wood-bundles. It consists not of two, but of three, layers of cells of which the middle row is absorbed. The two remaining rows, still living and turgid, swell outwards with rounded surfaces, and so create a strain which snaps the fibres and vessels. These observers also induced leaf-fall artificially at midsummer, by placing a cut-branch in a box filled with moist air, and they found that after the fall of the leaf the cellular tissue of the vascular bundles whose ends are exposed on the leaf-scar becomes "merismatic," *i.e.*, undergoes cell-division, forms cork, and penetrates and fills up the ends of the vessels.

It is well to bear in mind that prolonged drought

will induce leaf-fall much as does a frost, and that a layer of cork is formed below the prickles on old stems of rose or bramble, and below twigs in some plants which shed these branches as others shed their leaves. On the other hand, if a branch be broken through early in summer, its leaves wither but do not fall, no absciss-layer being formed. Coppiced oaks or the clipped beeches and hornbeams in the hedges of nursery-gardens also retain their leaves, as if the energy and material used up in the formation of callus to heal the wounds caused by pruning-knife or shears left none for the formation of the usual absciss-layers. The pollard hornbeams of Epping Forest, which used to retain their withered foliage through the winter, have, since the Forest was taken over by the Corporation of London, and lopping has been stopped, been gradually regaining the deciduous character of "spear" trees.

Everyone must have noticed the successive fall of the leaflets and the leaf-stalks in the ash or horse-chestnut, the thick-ended petioles being aptly known by children as "bones," since they are by no means unlike the leg-bones of birds. There is, however, another interesting little point in connection with leaf-fall which is, perhaps, less familiar, and which is well illustrated in the photographs, by Mr. Johnson, of Tunbridge Wells, from which our blocks have been prepared. This is the order in which the leaves fall from the twig. In the beech this is *basipetal, i.e.*, the younger leaves at the apex of the twigs fall first. In the linden, the poplar, and apparently in the majority of trees the fall takes place *acropetally, i.e.*, the older leaves at the base of the twigs fall first.

It is well to notice that here again we have order and not chance: that Nature has, as we often find, two or more ways of bringing about the same result; and that even in such an apparently simple matter as the fall of the leaf there is room for a good deal more research.

SOLAR DISTURBANCES DURING NOVEMBER, 1910.

By FRANK C. DENNETT.

DURING November there has been a considerable falling off in solar energy, a complete absence of dark spots being recorded on sixteen days, and on five of these faculic disturbances were also absent. It is a little peculiar that the majority of the disturbances have occurred in the northern zone, which for so long has been comparatively quiet. The longitude of the Central Meridian at noon on November the 1st was 18° 22′.

As No. 82 continued on the disc until November the 3rd it is shown on the accompanying chart.

No. 83.—A pore in the northern spot-zone, only seen on the 3rd.

No. 84.—Two pores only visible on the 7th, in the area formerly covered by No. 75.

No. 85.—A solitary pore also ephemeral, only being seen on the 14th.

No. 86.—First seen on the 16th, when it showed some of the characteristics of an elliptical outbreak. The leader, or western spot was 10,000 miles from north to south, and the group about 3° in length. The spectroscope showed the C line of hydrogen displaced both on the red and violet sides, and prominences were visible in projection over the group, whilst the D_3 line of helium was dark through the outbreak. On the 17th it was seen as a nearly straight group of small spots 40,000 miles in length. By the 18th it had assumed a lozenge shaped arrangement of pores, with the largest behind, or eastward, and when last seen on the 19th there were only two pores, 26,000 miles apart.

No. 86*a*.—A single, not very black, pore a little west and south of the group, only recorded on the 16th.

No. 86*b*.—A minute pair of pores 19,000 miles apart, in the former area of No. 78, only seen on the 18th.

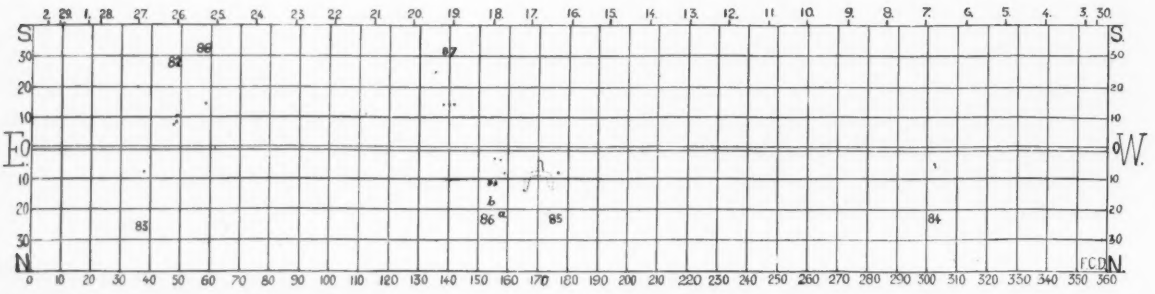
No. 87.—Three pores nearly in line, also seen on the 18th only, in the area of No. 79*c*.

No. 88.—A single spotlet seen from the 20th until the 22nd in a disturbed area, having apparently come round the limb.

On November the 12th a curved faculic ridge of horseshoe form was seen in northern latitude in the position dotted upon the chart.

The chart is constructed from the combined observations of Messrs. J. McHarg, A. A. Buss, E. E. Peacock, and F. C. Dennett, working in places so far apart as Lisburn, Chorlton-cum-Hardy, Bath, and Hackney, thus making the record almost complete.

DAY OF NOVEMBER.



EXPERIMENTAL MECHANICS.

By W. D. EGGAR, M.A.

MECHANICS is the mother of two sciences, Engineering and Gravitational Astronomy. This statement is perhaps open to question. Both engineers and astronomers may wish to claim a more remote ancestry for their studies. But, without insisting on the precise relationship of the different groups, we may point out that Gravitational Astronomy is very much younger than mechanics, and it might almost be said that she made her entry into the world after the manner of Minerva, appearing full grown and fully equipped from the brain of the god-like Newton The perfection of her equipment, and the unexceptionable propriety of her demeanour commended her to all mathematicians, with the result that Mechanics, her real mother, was received into their select circle, taking the name of Applied Mathematics, and not encouraged to see much of her other daughter, fond, it was feared, of low and irregular company.

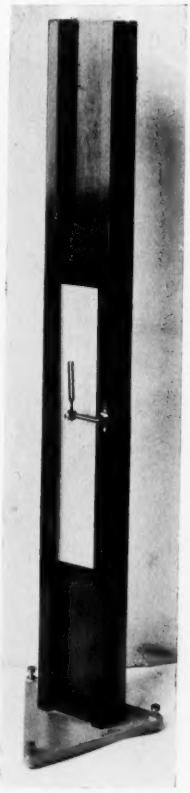


FIGURE 1.
Apparatus for measuring time with a tuning fork.

Times have changed. Even the House of Lords must become more democratic, and engineering has become so very much of a great lady as to compel the respectful attitude of mathematics, and to make her connection with trade rather a recommendation than otherwise. In other words, the world must have engineers, and engineers must have mathematics, and, therefore, mathematics has become more practical. Moreover, in the engineering profession itself the problems which require solution become more and more of a kinetic nature. The strains and stresses in girders and embankments are as important as ever; but, in addition, the mechanics of movement is growing in importance. We move faster nowadays, not only on the railways, but on the roads, in the water, and even in the air. Hence it is that the practical teaching of mechanics, which used to be limited to a few experiments with spring balances and pulleys, has now spread into the region of kinetics. The concepts of force and work are undoubtedly best approached by practical measurements of the efficiency of simple or complex machines; and it has been found that velocity, acceleration, kinetic energy, moment of momentum, and other concepts involving movement can best be realized in a practical manner.

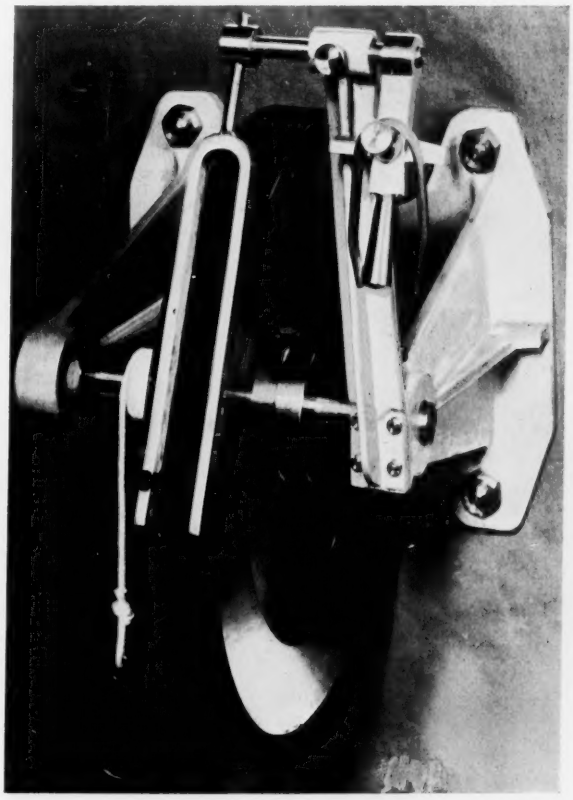


FIGURE 2.
Measuring the velocity of the rim of a wheel.

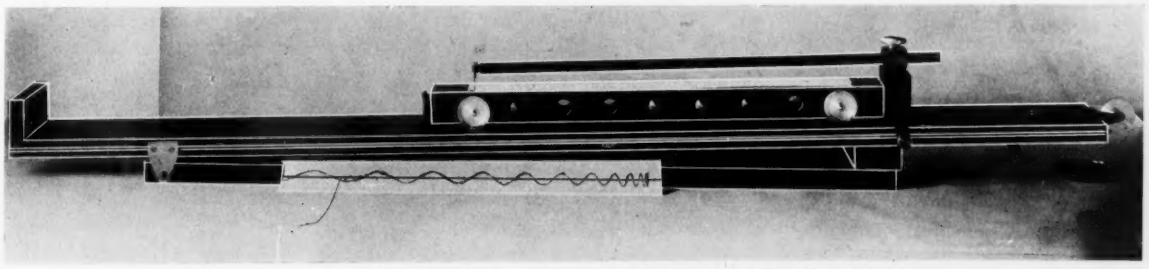


FIGURE 3. Fletcher's trolley for measuring acceleration.

The measurement of time has always been a difficulty in the way of those who have tried to teach kinetics by experiment. Electric chronographs are expensive, and, even where expense is no bar,

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they are frequently found wanting either in their working or in their power of elucidating the problems in which they are employed. A recent adaptation of the old tuning-fork method of measuring time has revolutionized schools of dynamics. The tuning-fork method will be best understood from a glance at Figure 1, which shows an arrangement for allowing a plate of glass to fall so as always to be in contact with a style attached to one prong of a tuning-fork vibrating with known frequency. A wavy-curve is traced on the glass by the vibrating point, and the space fallen in a given time can be determined by measuring the length of that portion of the wavy curve which contains a number of waves corresponding to the given time. Another apparatus for measuring the velocity of the rim of a flywheel by means of a tuning-fork is shown in Figure 2. Now a tuning-fork is not very easy to manipulate, and it goes rather too fast for beginners. Here it is that a great simplification has been made by Mr. W. C. Fletcher, of the Board of Education.* A strip of steel clamped at one end carries a paint brush at the other end. A long trolley carrying a strip of paper moves underneath this paint brush in the direction of the steel rod's length. Suppose the steel to vibrate ten times a second, then the paint brush will trace a wavy line on the paper, and ten waves will represent the distance travelled in one second. The lengths of the waves give the velocities, and the changes in the wave lengths give the accelerations. Figure 3 gives an example of the way in which Fletcher's trolley may be employed for measuring the acceleration when the trolley is allowed to run down an incline. One of the curve tracings made by the paint brush is shown resting by the side of the plane. As for the way in which the acceleration is determined from a curve of this kind, let us consider the curve shown in Figure 4. Placing a centimetre scale along its wave crests, we can read off the distances of these crests from the starting point of the curve to the nearest half millimetre, thus 0·1, 0·35, 0·8, 1·45, 2·25, 3·2, 4·35, 5·65, 7·1, 8·7, 10·5, 12·4, 14·5, 16·75. From this, by subtraction, we obtain the successive wave-lengths, viz.: 0·25, 0·45, 0·65, 0·8, 0·95, 1·15, 1·3, 1·45, 1·6, 1·8, 1·9, 2·1, 2·25. It is obvious from this that the wave length has been increasing fairly uniformly, the successive increments in wave-length being again obtained by subtraction. Thus they are 0·2, 0·2, 0·15, 0·15, 0·2, 0·15, 0·15, 0·15, 0·2, 0·1, 0·2, 0·15. The average of all these is 0·167. Now, since the wave-lengths correspond to periods of one-fifth second, it is plain that the average increase in the space travelled in each one-fifth second is increasing at the rate of 0·167 centimetre in every one-fifth second. Hence in each second the increase in the velocity is $5 \times 0{\cdot}167$ centimetre per one-fifth second, or $5 \times 5 \times 0{\cdot}167$ centimetres per second. The acceleration is therefore 4·17 centimetres per second per second.

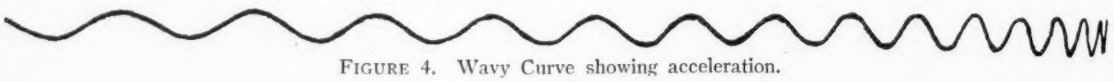

FIGURE 4. Wavy Curve showing acceleration.

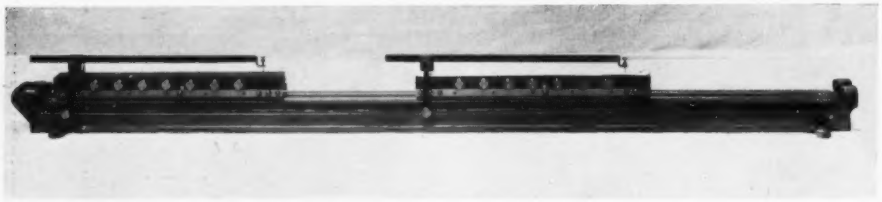

FIGURE 5. Two trolleys for verifying the laws of momentum and impact.

As an example of the way in which the apparatus may be employed we may take the verification of the law "Acceleration is proportional to the accelerating force." The variable force is obtained by varying the slope of the plane. To avoid having to consider the force of friction, the trolley is first connected by a string passing over a pulley, shown in the figure, to a scale pan of known mass, and weights are added just enough to prevent the trolley from accelerating when it is given a start down the plane. When the waves made by the paint brush are of equal length the total force down

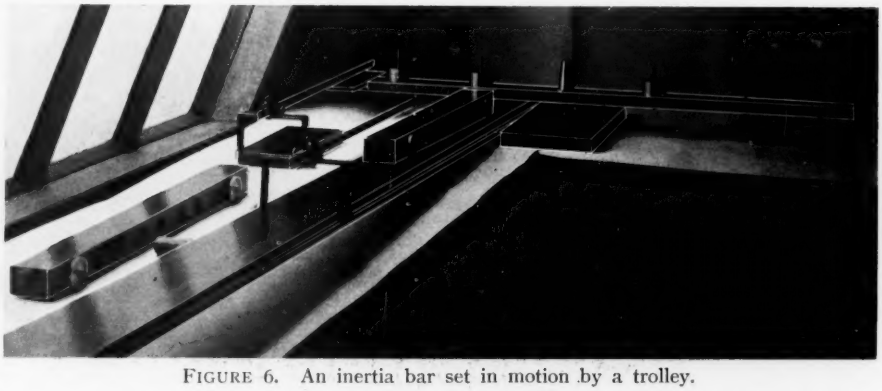

FIGURE 6. An inertia bar set in motion by a trolley.

* See *School World*, May, 1904.

the plane is exactly balanced by the scale pan and weights. Now if the string is removed the trolley will accelerate under a force which is equal and opposite to that which has been removed, viz., the weight of the scale pan and its contents. The acceleration can be measured as before. If the bullet embeds itself. After the shot the trolleys retire with equal momenta.

Figure 6 shows an inertia bar, which is set in rotation by the impact of a trolley. Vibrating springs are employed as before to measure the momentum of the trolley, and the angular momentum of the bar.

FIGURE 7.
A vibrator used for measuring the angular velocity of a fly wheel.

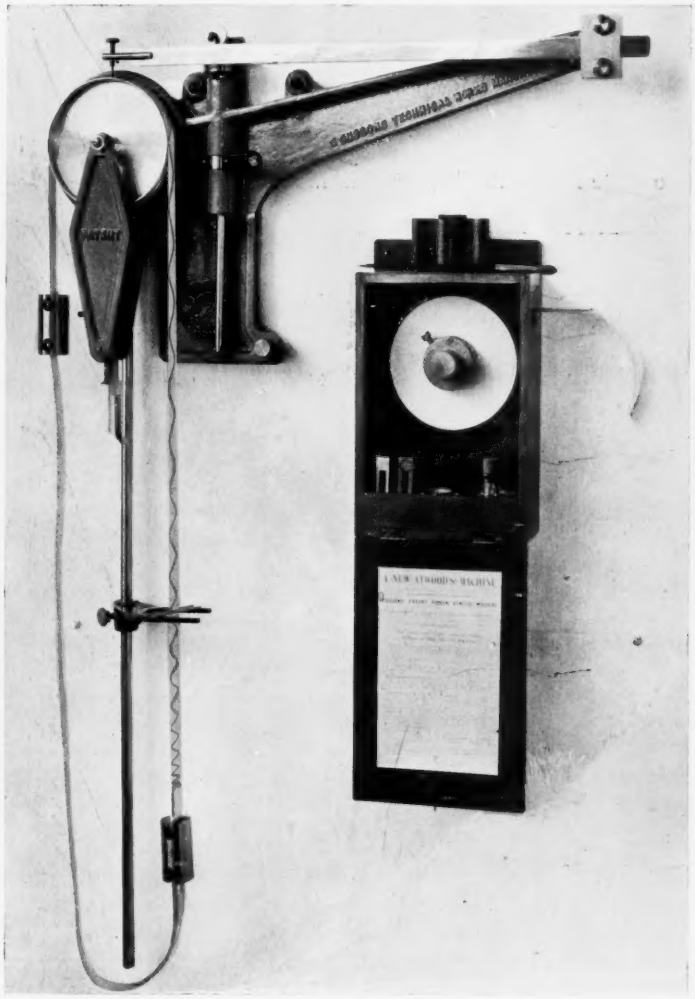

FIGURE 8.
The vibrator adapted to an Atwood's machine in which a paper ribbon replaces the string.

observations are now repeated with a different angle of slope the two accelerations will be found to be in the same ratio as the two accelerating forces.* An approximate verification of Newton's Second Law may seem unsatisfactory; but the method has the merit of making the notion of acceleration much more easy for the ordinary mind to grasp.

Figure 5 shows how, by means of two of these trolleys, laws of momentum and impact may be verified. In one form of the apparatus, employed by Mr. Ashford at Dartmouth Naval College, one trolley carries a pistol and the other a target in which the

Figure 7 shows the vibrator employed in measuring the angular velocity of a fly wheel. The notion of kinetic energy becomes clearer after a series of measurements taken with an apparatus of this kind.

Mr. G. Cussons, of Manchester, to whom we are indebted for the photographs used in illustrating this article, has adapted the vibrator to a form of Atwood's machine in which a paper ribbon replaces the usual string. The apparatus is shown in Figure 8. Excellent results are obtained from this instrument, and Figure 4 is a reproduction of one of the curves obtained from it.

* See Eggar's Mechanics, E. Arnold.

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CARRIERS OF PLAGUE.

By E. S. GREW.

With illustrations from photographs by the courtesy of Dr. C. J. Martin, F.R.S.

TECHNICALLY any animal is a carrier of plague which has plague, and in the organs or blood of which the plague bacillus resides. In that sense the natives of India are plague carriers; the rats and bandicoots of India are still more emphatically plague carriers. But, in the last resort, seeing that the instances in which a rat suffering from plague could inoculate man with plague bacilli must be so rare as not to be worth considering, the true carriers of plague are the animals which convey the bacilli from rat to rat, or from rat to man, or, in rare instances, from man to rat. These animals are the fleas which infest rats.

FIGURE 1.
An old Bengalpura Street.
All the dwelling houses are above stables, and therefore, on the face of it, likely to be infested with rats.

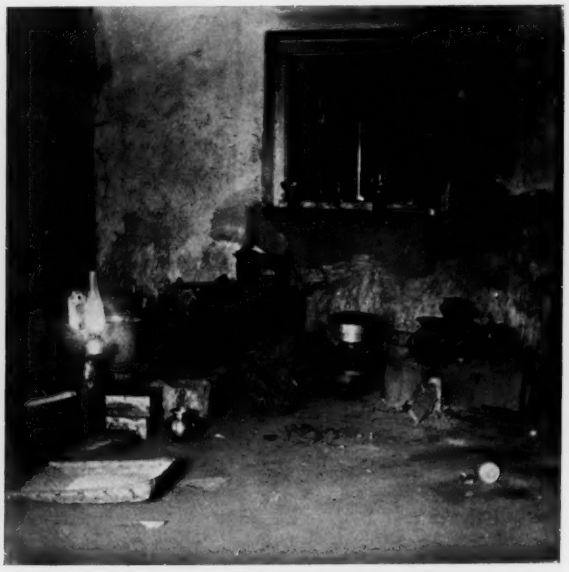

FIGURE 2.
The interior of a Cooly's Room.
Pots and boxes containing articles of daily diet are lying all over the place and affording shelter and facility to rats.

This conclusion, apparently so simple, was, however, not reached without long continued investigations, undertaken, at the instance of the Indian Government, by a commission of bacteriologists (under the direction of Dr. C. J. Martin, F.R.S., and Col. David Bruce, F.R.S.), which has been at work since 1904. Among the investigations made by the Plague Commission's bacteriologists have been a number which dispose of the idea that plague can ordinarily be spread by plague bacilli which may be left on the earth—on the floor of infected houses, or on the soil. Nor can plague be transmitted usually by aërial infection. That is to say, it cannot be transported through the air, or with wind-swept dust. Nor, again, though this belief has had a long currency, can it usually be transmitted through food. *The Indian Plague Commission have discovered no fact which would support the suggestion that plague may effect an entrance into the human organism through the stomach or intestinal canal.* The probabilities of infection through swallowing any number of plague bacilli are extremely small; and it is justifiable to conclude that in nature, infection of rats by feeding rarely or never takes place, *and that rats do not become infected by eating the carcases of their comrades.* The repetition of these facts has a certain interest at a time when the appearance of plague among rats in Suffolk has created a very distinct prejudice against the use of game, of hares, and even of imported rabbits as food. Everyone would, of course, prefer not to eat any animal which could possibly have died from plague, but no one would be in the least likely to contract plague by doing so.

In view, however, of the fact which is now declared by the highest authority to be well established, that there have occurred two cases of plague pneumonia in Suffolk, and that there have been a number of cases of plague among rats in that neighbourhood, it becomes important to inquire what the probabilities are of any considerable spread of the infection in England.

In any outbreak of plague in a new locality the factors which determine the extent and severity of the epidemic are much more numerous than are generally supposed, and the margin between danger

and safety is dependent on factors which are in themselves apparently slight. For example, it has already been said that plague is spread entirely by plague fleas. This may naturally lead to the inquiry

FIGURE 3.

The house (at Parel) with the plant pot in front, produced a very large number of rats.

whether the last Great Plague of London, which proved so devastating, was spread by fleas? If it

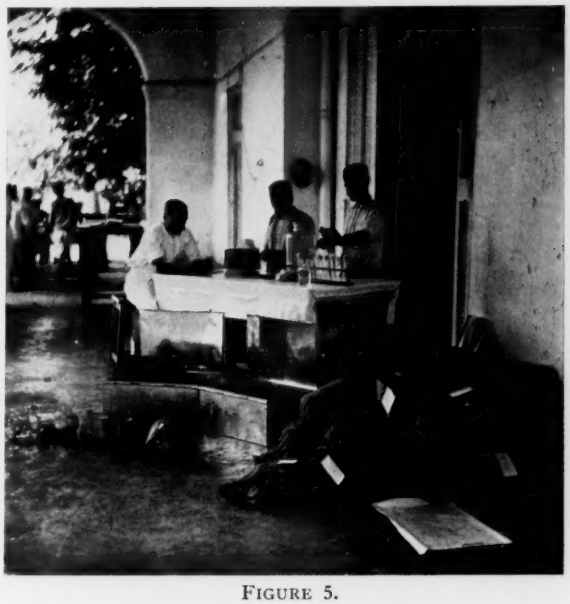

FIGURE 5.

Taking a systematic flea count.

were so would not this argue that the habits and dwellings of London in the late seventeenth century were much more dirty than we have any evidence to show that they were? Moreover, would not the supposition be in conflict with the recorded facts that whole families were stricken with the plague, apparently taking it from one another? That is certainly true; and the answer to these questions is

FIGURE 4.

An examination of plague infected rats at one of the Indian Government Laboratories.

to be sought in the fact that plague has more than one development. It may develop into plague

FIGURE 6.

Picking off fleas from live rats for experiments.

pneumonia, which, of all forms of plague, is the most dangerous and the most infectious. The mortality in plague pneumonia approaches one hundred per cent.; hardly anyone recovers from it. Moreover,

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while suffering from it the patients are delirious, they are with difficulty restrained from walking about, and they cough and spit incessantly. Their sputum is full of plague germs, and is highly infectious to anyone on whom it may be discharged. Rats, also, may have plague pneumonia; and there is one instance at any rate in which a bacteriologist is believed to have contracted plague from the sputum of a plague infected rat. The Great Plague of London took place in winter, and a large number of victims no doubt had plague pneumonia; hence the virulence and rapidity of the contagion.

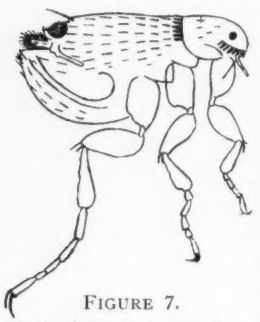

FIGURE 7.
Pulex felis ♂, the cat flea.

What then is the genesis and progress of an outbreak of plague? Plague, in the first instance, appears to arise in certain foci in Asia and Africa, where it always exists. In a locality such as Bombay, an outbreak among human beings is preceded by an outbreak among rats, and if a curve be drawn showing the rise, culmination and decline of the plague among rats, it is found that a rise, culmination and fall of plague among human beings takes place in a curve almost parallel to that of the plague among rats, but occurring about a fortnight later. The reason is quite plain. In the native dwellings of India generally, and in the native villages of the Punjab, man and the rat live together like friends. There are two kinds of rats in India, as there are two kinds of rats in England, though the numerical proportions of the two kinds are usually reversed. In England the commonest rat is the so-called grey rat, *Mus decumanus*, or Hanoverian rat, which usually inhabits sewers, and is a fierce, strong animal, shy of human beings. In India the commonest rat is the black rat, *Mus rattus*, which is not at all shy, and which, being allowed to do so, lives on terms of intimacy with the Indian native. But the black rat is found in England, in the Thames and Mersey warehouses; and the grey rat is found in Indian ports. The reason why the black rat spreads plague in India is that it dwells unmolested in the native villages, and in the ramshackle native quarters of towns; and that, consequently, if it has any epizoötic disease which can be transmitted to man, there is an initial probability of its transmission. In short the Indian rat is a reservoir of plague, which is constantly being tapped by the fleas which live on it, and which carry it on to the human being in the rat's neighbourhood. On a rat infected with plague thirty fleas are by no means an excessive number. In one native house in Parel, an Indian village, no fewer than three hundred rats were trapped. Multiply the number of fleas by the number of rats, and the number of possible plague inoculators thus arrived at will give an indication of the risk which Indian natives run of contracting plague when there is an outbreak of it among rats.

FIGURE 8.
Sarcopsylla gallinacea ♂, the chicken flea.

FIGURE 9.
Micro-photograph of a ♂ *Pulex cheopis*, the Indian rat flea.

It will be seen, therefore, that even if there were an outbreak of plague among the rats of Suffolk, the chances of the development of a corresponding outbreak are not very large. But they are further narrowed by the kind of fleas which live on rats in this country and in these latitudes. There are six kinds of fleas which have been found on rats. The human flea *(Pulex irritans)* is found on it sometimes, but not very often. The dog flea *(Pulex canis)* is found more often but still seldom. The mouse flea (*Ctenopsylla musculi*) (see Figure 10) is also found.

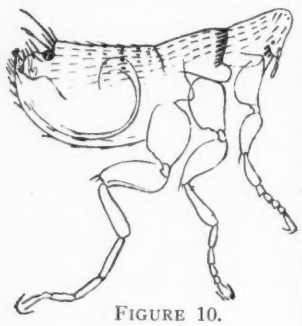

FIGURE 10.
Ctenopsylla musculi ♂, the mouse flea.

There are, however, three kinds of fleas which are found commonly on rats, and the consideration of which is more particularly relevant to the question of the contagiousness of plague. The first is a rat flea, *Typhtopsylla musculi* (not unlike the mouse flea), which is common on rats in some parts of Europe; but which will not bite man. The second is the most common European rat flea, *Ceratopsyllus fasciatus* (see Figure 11). But this flea will not feed on man, except when starving. The last, and most important, flea is *Pulex cheopis* (see Figure 9), the Indian rat flea, which, unlike the European rat flea, readily feeds on a number of

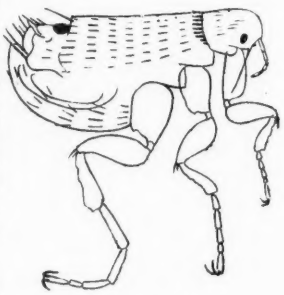

FIGURE 11.
Ceratopsyllus fasciatus ♂, the European rat flea.

animals. In the absence of rats it will readily bite man.

It will be seen at once how important this factor is. The varying appetite of the different kinds of fleas modifies to a great extent the probability of the transmission of plague to man. If it were possible, or probable, that the European rat flea should alter its habits and adopt man as a host, the probability of human plague in Europe would be of an entirely different order. At present northern Europe, and, to a less extent, Europe in the temperate zone, seems immune from an outbreak of plague on account of the habits both of the grey rat and of the flea which lives on it.

CORRESPONDENCE.

PODURA SCALES.

To the Editors of "KNOWLEDGE."

SIRS,—While fully appreciating Mr. Plaskitt's kind reference to my article, for which I thank him, there are yet certain points in his letter to yourselves with which I, for one, cannot agree. I cannot admit, for instance, that in the Podura scale mounted dry we are "left with the only alternative (that is, in being mounted in a medium) between the normal of air and the scale itself"; or, that for that reason, "we make no advance upon the earliest methods." As I understand it, with a scale mounted dry, and tight against the cover glass, there is no normal of air, but perfect optical contact with glass throughout, also with the cedar oil of the same refractive index between that and the objective. Mr. Plaskitt speaks of the scale, not being flat, giving rise to distortion. That has not been my experience, though it may be said of other kinds of scales, such as those belonging to the Moth and Butterfly.

Assuming that the widest apertured oil immersion objective is employed upon this scale, the best way to discover whether it is working at its maximum is to look down the tube of the microscope after removing the eye-piece. If the back lens is seen then to be filled as to three-quarters of its diameter with white light, this condition may be said to be fulfilled. Under no circumstances could this be obtained with an objective of 1·40 N.A. were there a stratum of air between the object and the cover glass.

These conditions were fulfilled in all my work upon this scale. For myself I never required other proof of optical contact, than the flashing out of the object in brilliant light, as if the scale were self-luminous. Apart from this, however, my friend, Mr. E. M. Nelson, with whom, at that time, I spent many delightful days at his house over the microscope, tested it for his own satisfaction with the Vertical Illuminator, when the test was always justified.

As to the three-quarter cone of white light at the back lens of a wide apertured objective; this for a long time now has been looked upon as the ideal illumination, where the greatest resolving power is combined with perfect definition. Mr. Nelson, in his Presidential Address to the Quekett Club in the early part of 1895, went fully into this question. The address will be found in Vol. 6 of the Journal of that Club, beginning at page 14, but as some readers may not have access to it, I beg to be allowed to make the following extract for their benefit:—

"It is common knowledge that when a full cone is employed, the resolving power falls off, and it has been customary to account for this falling off in the resolving power by the outstanding spherical aberration in the objective. To test the accuracy of this current notion a critical image was set up, and matters arranged so that access could be obtained to the back lens of the objective without disturbing any of the adjustments. When a full cone of light was used (that is, the back lens entirely filled with light—T.F.S.) the resolving power fell off, and when a three-quarter cone was employed it was as usual restored again; a stop was then placed at the back lens, cutting off the peripheral unilluminated annulus. We had, therefore, an objective of less aperture, but illuminated by a full cone. Under these circumstances one would have expected to see a critical image, but not so, and this is the crucial point. In order to obtain the maximum resolving power for that reduced aperture the illuminating cone had to be reduced until three-quarters of the back lens was illuminated."

It follows, then, that given the three-quarter-cone as the most ideal illuminant, such a cone of an oil immersion objective of 1·40 N.A. must have a much greater resolving power than the similar cone of a dry objective whose maximum aperture is only 1·0. Up to a certain period dry glasses were the only ones available. All my photographs of the scale were taken under the first conditions; hence, unless I blundered greatly, they must have made an "advance upon the very earliest methods."

We now come to the question of oblique light as helping towards the further elucidation of structure in the Podura scale. Mr. Plaskitt says, that when central light is used, "there is insufficient contrast to bring out several of the finer phases of detail. It is much the same as if one placed a piece of plain glass in the midst of a lamp flame and were asked to observe its uneven surface, or to direct a telescope boldly towards the full moon and be expected to trace its finest details."

The illustrations on the face of them are apt enough, yet, I fear for all that, a little misleading. The conditions of producing an image in the microscope differ from those concerned with unaided vision upon natural objects. In the microscope it is of a two-fold character; one geometrically, as in ordinary vision, taking up the centre of the back lens of the objective; the other, by diffraction, occupying the outer unilluminated ring. Both images reach the eye as one, that produced by diffraction strengthening the image produced in the ordinary way of vision. Neither with regard to the telescope and the moon do I think the cases are exactly upon all fours with each other. In one, the microscope, the light is tilted to the object; in the other the telescope remains the same—the moon it is that revolves. Mr. Plaskitt's photographs do not exhibit shadows such as when details in the moon are seen in profile, proving, to my mind, that the conditions of production are not the same.

Like Mr. Plaskitt I have been unable to discover any difference between the two sides of the scale, though I have had them mounted between two thin cover glasses, to enable me to get at each side. It is, indeed, altogether a puzzle, this secondary structure, and such for the present, I fear, it must remain. One thing, however, I would say in conclusion. If my own experience with minute structure is any guide, and with all deference I say this, it is that oblique light will not guide us further, but will only make confusion worse confounded.

Yours faithfully,

T. F. SMITH.

XUM

ON THE OCCURRENCE OF SOME SMALL FINELY WORKED FLINTS IN PALEOLITHIC GRAVELS.

By LIEUT.-COL. UNDERWOOD.

AT various times during the past two years I have been carefully searching two gravel pits in which paleoliths are found—one within a mile of Ipswich, the other at Upper Dovercourt, near Harwich.

The gravels of these two pits are of entirely different ages—that at Dovercourt being a terrace gravel of the River Stour, and situated eighty-seven feet immediately above it; while the gravel at Ipswich is not apparently connected with any river system, and as it rests on London clay (the base bed) and is surmounted with glacial boulder clay and interlocated with fine sand containing quartz pebbles, rounded diorites, sarsen stones and other erratics, it may be assigned to Middle Glacial age, and is undoubtedly much older than the Dovercourt deposit. In this latter pit no regular Acheullien *haches* are found, as at Dovercourt, and no large implements of war, until we come to the lowest bed of red sand, with large blocks of flint, and a few other stones, similar to what Rutot calls the "*gravier du fond*," found by him at Greville, which lies directly on the base bed, or London clay, where we get very large parallel-flaked, rough-pointed "*Coups du poing*" rubbers, and so on, but no small side-worked tools.

From Gant's Pit, Dovercourt.

The Dovercourt pit, on the other hand, consists of a mass of gravels and sands resting on the London clay to an average depth of twelve feet. These are divided by a band of clay about half way down, of a few inches in thickness, which separates the bones and implements into two distinct zones. In the upper beds are found the remains of deer of four species:—Reindeer, Red Deer, and Fallow, this species of a large kind which has also been found at Clacton. Boar, Wolf and Arctic Fox bones, occur with implements mostly unpatinated, or only slightly so, of Mousterian type, consisting of small tools, from three to five inches long, assegais, side knives or choppers, dressed flakes, rubbers, scrapers (round and hollow) and dressed bones, and a fish hook made from a halibut's clavicle.

In the bed below the clay band are found the bones of Mammoth, two kinds of Rhinoceros, *Bison briscus*, *Bos primigenus*, and the Horse, while the implements are of late Acheullien type, large and beautifully worked haches, together with small tetragonal and hexagonal borders, dressed flakes, rubbers of a peculiar kind, and a series of very small side-worked flints of almost pigmy appearance, consisting of delicate hollow and round scrapers, saws, and very small pointed tools like arrow or dart heads. These latter are so finely chipped that it is necessary to go to specimens of late Neolithic culture to find their equals, and though this is so, their deep patination and scratched surfaces point to an antiquity far greater than the Acheullien *haches*, which are lying *in situ*, un-water worn, while these small types, with some large and rude forms, have evidently been washed in from an older gravel. In the gravel at Ipswich exactly similar diminutive implements occur at depths ranging from ten to thirty feet from the surface, but are considerably more numerous.

From Bolton and Laughlin's Pit, Ipswich.

The large number of these small well-worked implements which my friend, Mr. Moir, and myself have collected is very remarkable, and brings into prominence some questions which have not hitherto been discussed. Firstly: How is it that in our Museums, many of which contain large collections

of magnificent weapons of War and the Chase from the principal river gravels, these paleoliths are seldom shewn? It is almost a certainty that they occur everywhere, but owing to the fact that workmen are only paid and encouraged to look for the larger specimens, presumably they are generally overlooked.

Secondly: These small fabrications are not to be found in the later gravels, as in the case of Dovercourt, above the clay band, nor are they found associated with large early Neolithic implements of Cisbury and Eastbourne. This seems to point to one of two conclusions, either that side-pressure flaking or "nicking" became a lost art at one early period and was not resuscitated till later Neolithic times; or that the workers (possibly a special race) of so-called "pigmy" implements and small side work generally were driven from districts where their ancestors made these "industrial tools," as Rutot so aptly describes them, and did not again migrate to the same places for many ages later.

Thirdly: I would suggest more light would be thrown on the present mystery of patination, if a general system were adopted of classing the various implements in all the gravels of England according to their forms and respective ages; and the gaps in the evolution of man's handiwork and arts would be more filled up by such a system than at present.

In the accompanying illustrations the finds of the two pits respectively are shown in types, and with the name of the pit in which each was found.

THE MYSTERY OF THE FLORAL PIGMENTS.

By P. Q. KEEGAN, LL.D.

THE literature on the subject of the colours of flowers has been very voluminous as respects the final cause of the phenomena, but a truly scientific explanation thereof has seldom been attempted. We do not want to discover the purpose which these lovely pigments fulfil in the economy of the plant, so much as the chemical and physiological efficient causes of their appearance. The chemistry of plants requires to be studied, and the products of the physiological processes going on in their interior need careful detection and distinguishment. In this way only can the laws which govern the vital activity be discovered, and thereby also a true scientific understanding of the phenomena be attained. The corolla is the organ wherein the yellow, blue, and red colouring matters are chiefly produced, and hence its physiology has to be carefully studied and determined, not only in itself, but also in its relationship with that of its neigbouring organs, viz., the stamens and pistils. We know that the corolla grows very rapidly, that it is the seat of energetic oxidation and at times considerable transpiration; its assimilatory power is generally feeble, and it has a poor development of the vascular system, *i.e.*, of the conducting apparatus. Hence the appearance of the pigments in the corolla has been explained in a general and rather vague way by simple modifications in the phenomena of cellular nutrition and of cellular chemistry (Curtel[*]). Chlorophyll is not produced or regenerated in the corolla owing to insufficient nutrition, and hence carotin (the pigment of the orange and deep yellow flowers) is allowed to appear, while the blue and red pigments are due to the energetic oxidation of which this organ is the special seat. This explanation seems to account fairly well for the production of the yellow pigment (carotin), but it fails to throw light on the fact that the red and blue pigments (anthocyan) are evidently evolved by a very special physiological process, which does not generally take place in any other part of the plant with a strength and completion at all comparable with that proceeding in the tissues of the corolla. The mere oxidation of an organ is not sufficient, unless something is produced there independently, whereon the oxidation may operate. If the corolla possesses a very feeble or annulled assimilatory power, its protoplasm exhibits a very energetic deassimilatory power, and this latter fact is proved to demonstration when the chemical constituents of the floral parts are compared with those of the other organs of the same plant.

A series of chemical analyses of common plants performed by myself (see *The Naturalist*, 1902–10), revealed the important fact that, while the flowers of certain species are habitually tinted purely and vividly, the quantity of tannic chromogen contained in the other organs (stem, leaf, root) is or may be, extremely small. Hence, the conclusion was readily suggested that the formation and development of the blue and red pigments in these cases were strictly local, and absolutely confined to the floral envelopes, *i.e.*, they were not necessarily dependent on the particular amount of tannin produced by the organism in its entirety. Now, as tannin is a product of deassimilation of a high grade, it seemed certain that this particular physiological process was carried on in the corolla very energetically; it was pushed further there, as it were, than in the other organs of the plant. The question then to be decided was in what manner did it act, and what also were the real causes of its specific energy and completeness.

It became clear that it was not (as M. Curtel opined) merely because the corolla possessed feeble powers of assimilation, that, therefore, its powers of deassimilation should be raised correspondingly. The law of plant physiology applicable to this case is that processes of special deassimilation are brought about in certain cells to the profit and benefit of other neighbouring cells. The albuminoids of the corolla cells minister to the pressing needs of the stamens and pistils. The vital activity incident to the process of fecundation, the formation and differentiation of the stamens, ovules and seeds, etc., induces a powerful drain on the albuminoids of the corolla, whose molecules consequently break up; the nitrogenous nuclei thereof are separated and pass over as much as is required to the cells of the stamen and pistil, while the aromatic nuclei remain behind as tannic chromogen, the precursor of the brilliant blue and red pigments. Numerous experiments performed by myself (see *Nature*, lxi., 105) render it certain that the conversion of tannin into visible pigment varies in intensity and completeness very considerably, according to the particular petal examined. In cranesbill, tufted vetch, peony, and sweet pea the conversion is complete; in foxglove, carnation, and some roses it is nearly so; in clover, sea pink, and flowering currant it is not complete; while poppy, burnet and cineraria are still further remote from perfect development of tincture. And, inasmuch as different species of plants belonging to the same genus evolve habitually either blue or red flowers, while the chromogen of all is exactly the same substance, there can be no doubt that these differences can only be explained by a diversity in the development of the pigment. As already suggested, the most developed in this respect are those plants which habitually evolve blue or purplish-blue petals, and it is these very plants which exhibit the most intense and energetic reproductive capacity.

[*] *Annales des Sciences Naturelles.*—Bot. Ser. 8, t. 6, page 221.

XUM

ON THE STUDY OF DOUBLE STARS BY AMATEUR OBSERVERS. V.

By G. F. CHAMBERS, F.R.A.S.

PRACTICAL HINTS ON OBSERVING DOUBLE STARS.*

DIMENSIONS of telescopes in inches of aperture must not be regarded as conclusive as to the possibilities or impossibilities of dividing close Double Stars. There are several factors of importance involved in the matter: *e.g.*, the quality of the object-glass; the steadiness of the mounting; the power of the observer's eye; its training to detect small points of light; his skill and experience generally. How important such points as these are is well illustrated by a remark by Burnham, made many many years ago, when he presented one of his early catalogues of new Double Stars to the Royal Astronomical Society.† Speaking of some measures which had been placed at his disposal by Baron Dembowski he says:—"Few observers would be able to measure such stars, and those habitually observed by him, with an aperture of only 7 inches. It is another illustration of the truth that much more depends upon the observer than the size of the instrument. I may add that nothing has been more gratifying and flattering to me than to find my measures of a difficult pair with 18½ inches aperture agreeing closely with the measures of Baron Dembowski with 7 inches."

Another illustration of the proof of Burnham's general statement here is to be found in the Double Star work done many years ago micrometrically by Dawes, and in another aspect (the literary one) by Webb. The form of micrometer best adapted for general use is that known as the Bifilar micrometer mounted in conjunction with a position circle. For the benefit of beginners in the use of such an instrument it is suggested that after bringing a Double Star into the field of view the milled head which moves the position circle be turned until the position wires are approximately parallel to an imaginary line joining the stars. The stars are then by means of the slow motion on the telescope to be brought between the position wires, and then a final adjustment of the wires to parallelism with the aforesaid imaginary line is to be made. The distance wires are then to be approximately adjusted to the distance of the stars, and then by means of the slow motion of the telescope the stars are to be brought on these wires at a place clear of the position wires, and the final rectification made. It will be found most convenient to make these measures thus at places on the wires where they do not cross the position wires. Two readings, one of position and one of distance, are then to be taken and entered on a printed blank form of which a specimen will be given presently. It is advisable to take at least four measures of each sort, and treat the mean of the four as the result to be placed on record as the measures of the star observed.

In measuring Double Stars it is important that the observer should so hold his head that the line joining his eyes should either be coïncident in position with the line joining the stars or directly at right angles to it. Carrying out this suggestion may sometimes involve a strain on the muscles of the neck which must be guarded against, and this will best be done by using a diagonal reflecting prism on the eye-end of the micrometer. The angle of position required is that obtained by measuring round the circumference of a circle (with the principal star of the pair in the centre) from the N. point round to 360°, backwards contrary to the motion of the hands of a clock. The reading of the micrometer wires should be 0° and 360° when the wires are vertical. There is no test of the verticality of the wires which can be quickly applied, and when the instrument is attached to the telescope for use the position of the zero will be found to be slightly different from night to night. It is not worth while, therefore, to attempt to have the zero in the correct place. And after the micrometer is attached to the telescope preparatory to starting work, or during the observations, the angular error of the zero point out of place is obtained in the following manner and applied as a correction to the mean reading of the position angle of the star. The wires are brought approximately to point East and West and a star of any sort is brought into the field. The slow R.A. motion of the Equatorial is then used, and by trial the star is caused to travel along one of the position wires, the observer adjusting the position of the wires until the motion is along the wire. This done the wires then lie along a parallel of declination and are therefore displaced 90° from the N. point. Subtract 90° from this reading and we have the zero error. On the position circle there are usually two verniers 180° apart, so the observer has a choice of readings 180° apart; and, moreover, the wires will be in the same position on two stars really differing in angle by 180° so that it may be necessary to add 180° to the resulting measure. Whether or not this

* In the preparation of this section I have been materially helped by Mr. G. M. Seabroke, F.R.A.S.

† *Memoirs R.A.S.*, vol. xliv, page 147.

must be done must be settled by the estimated position of the star, or by consulting other previous measures.

Thus far we have spoken of taking measures of distance by adjusting the wires on the stars; and if the micrometer screw-head reads zero when the wires were over each other we should only have to convert the divisions of the reading on the stars into seconds of arc, and the angular distance would be had. It is not without risk of error that we can adjust the screw so as to read 0° when the wires are coïncident, and the difference in the expansion of the steel screw and the brass in which it is mounted would soon cause an error in the value of the zero which might, however, be allowed for when ascertained. But it is better to avoid this complication of error by taking measures of the star with the moveable wire first on one side of the fixed wire and then on the other. The distance through which the wire is moved will then be double the interval between the stars, and dividing this by two will give the actual interval; which process also has the good effect of halving the error of observation. Thus, it is advisable that the reading of the screw-head should only be approximately zero when the wires coïncide. If the readings are taken when they *increase* with the increase of the distance of the wires they are called "*direct*"; and when the moveable wire is on the other side and they *decrease* with the increase of the distance of the wires, they are called "*indirect.*" Since the numbers decrease from 100 (or from whatever may be the reading of the screw-head) on the indirect side we have to subtract the indirect reading from 100 and add this to the direct reading.

To avoid risk of error and for convenience' sake, it is well to adopt the fiction of calling the reading 1000 when the wires coïncide, that being the usual number of divisions for 10 revolutions of the micrometer screw. It follows, therefore, that all readings within this distance of the wires are *positive*, and we have only to subtract the smaller reading from the larger to get the double distance of the interval measured.

For instance: suppose that a direct reading embraces one revolution (= 100 divisions) and 15 more, making a total of 115 divisions. Add to this 1000, and write down the result = 1115. The indirect reading of the same stars after crossing the wires will also be one revolution (100 divisions) and about 15 divisions over, but as we are reading backwards from 100 the reading actually is about 85, say 80. (For it is not likely that the wires coincided exactly at 0). On the assumption that coïncidence takes place at 1000, we write down 880 as the indirect measure; then $\frac{1115-880}{2}$ is the distance of the stars in divisions of the instrument.

To find the value of the divisions in arc, separate the wires by a known number of revolutions, and note the time that a star on or near the equator takes in passing transit-fashion from one wire to the other. Then, since 15″ of arc are covered in 1ˢ of time the number of divisions (100 times the number of revolutions) divided by 15 times the time of passage in seconds will give the number of divisions which correspond to 1″ of arc.*

Seeing that it often happens that measures have to be reduced under circumstances unfavourable to accurate mathematical thought, it is well to resort when possible to mechanical labour-saving expedients.

One of these available for Double Star work is a "Barlow lens," the usefulness of which seems now-a-days to be very little understood. It is an achromatic plano-concave lens which has a powerful magnifying effect, and by inserting such a lens between the object-glass and the micrometer a few inches inwards from the latter an image is altered in size so that the distance between the two stars can be made to accommodate a certain number of micrometer divisions without fractions. For instance, the magnification of a pair of stars may be so adjusted that 10 divisions correspond to 1″ of arc. After finding the value in divisions of a measure we have only to move the decimal point, and then a value recorded in seconds of arc presents itself. Should this value not be absolutely correct we learn at any rate the percentage of the error and apply it in writing down the definitive distance.

The following forms are recommended for use. No. 1 shows the method of entering observations as they are taken, and No. 2 the method of recording the final results for permanent reference:—

No. 1. TEMPLE OBSERVATORY.

DOUBLE STARS.

Date: *Sept. 6, 1909·67.*

Σ *2218*

R. A. *17 39.* Dec. *+63 11.* Mag.

POSITION.		DISTANCE.	
Wires E. & W. *166·2*		Direct.	Indirect.
	90	*1017*	*982*
Zero error	*76·2*	*1018*	*981*
Readings.		*1017·5*	*981·5*
58·3		*981·5*	
57·0		2) *36·0*	
57·0		*18·0*	
58·5			
4) *230·8*			
57·7			
76·2			
161·5			

Pos. { A.B. *341·5* / A.C.

Dis. { A.B. *1·80* / A.C.

* A more exact method of obtaining this result is to measure the distance of two stars whose position is very exactly known such as certain stars in Pleiades. As to this see Gledhill's "Double Stars."

No. 2. TEMPLE OBSERVATORY.

	h. m. s.				
1880 Approx. R.A.	*17 39 35*			*1880* Approx. Decl.	*63° 43′*
Cor. for *1900*	*17 39 42*	Name.....	Mag^{s}. A. *6·5* B. *7·7* C.	Cor. for *1900*	*63° 44′*
		Struve's number *2218*			
		h „ *7137*			

Position.°	No. of Obs.	Dist.″	No. of Obs.	Date 180 +	Observer's Name.	Remarks on Observation.	Notes on previous measures, &c.		
348·2	*5*	*2·29*	*2*	*83·70*	*S*		*1832·7*	*2″·50*	*356°·7* Σ
349·8	*5*	*2·03*	*2*	*·70*	*S*				
349·2	*4*	*2·44*	*2*	*·72*	*S*				
341·5	*4*	*1·80*	*2*	*09·67*	*S*				

Gledhill has made the following suggestions as to Double Star observing which he says embody the recorded experience of Struve, Sir J. Herschel, Dawes, Dembowski, and Secchi :—

(1) At the outset it must be remarked that the Observatory (doors, windows, slit, and ventilators) should be thrown open at least an hour before observation begins, in order to reduce the temperature of the room to that of the external air.

(2) If the definition be bad and the motion great, it is useless to attempt the measurement of Double Stars. In short, if a power of at least 300 cannot be used, the results cannot generally be of any value.

(3) Very bright stars should be measured in daylight or twilight.

(4) The observations should be made near the meridian if possible.

(5) The observer should be in an easy position—the prism effectually secures this; and the driving clock ought to go smoothly.

(6) The bright-field should be used almost exclusively—red and blue colours are most in use.

(7) Use the highest powers possible, and always the *same* powers.

(8) A moderate number of measures of an object on each of two nights is better than a large number on one night.

(9) Use printed forms.

(10) Enter date, hour, weather, and distance from meridian, before observation begins.

(11) Notes on definition, general impression as to the value of each measure or set, etc., cannot well be too copious.

(12) In all doubtful cases make a sketch, and add full description.*

A few more miscellaneous hints applicable to telescopic work on Double Stars will now be given.

Use a dew-cap of a length equal to two or even three diameters of the object-glass; and it is well to have the dust-cap fit the end of the dew-cap as this saves moving the latter every time that work is begun.

Remove and carefully cap the micrometer every night to keep dust and spiders from getting on to the wires.

See that the Right Ascension and Declination of the stars which you are going to measure are for an epoch sufficiently recent that you can make sure of finding the star you want, or you may find yourself measuring the wrong star. The epoch of 1900 is sufficiently recent, and the principal star of every pair given in these articles is sufficiently bright that there should be no difficulty in dropping upon any star wanted; but in all cases of doubt the places may be brought up for Precession for any epoch later than 1900 by means of the Table of Precessions.

Examine by daylight the telescope and accessories and see that everything is ready for use in its place and in working order; or, come night and the right star brought into the field, and you may find some of the moving parts of the telescope or micrometer out of order and not be able to rectify matters by lamplight.

For reading a micrometer small electric lamps with a *pinch* contact are the best for use; but whatever lamp is employed, take care that the light is to such an extent shaded as not to endanger the sensitiveness of the eyes.

Take as many readings as you can remember before writing down any of them, but be sure you do remember them. It is a great advantage to have an assistant to write down the figures whilst the observer is calling them out. In this way the observer is able to keep his eyes unimpaired by too much contact with light. Small electric lamps of three candle power with switches fixed to the verniers of the setting circles are valuable accessories.

Have a movable slide with red, yellow and blue glasses fitted in front of the lamp which illuminates the field, and also a stop† to control the amount of the incoming light. In this way the illumination of the field can be readily adjusted. It is always well to use sufficient light to illuminate the wires even

* Handbook of Double Stars, page 83.

† A slide of smoky glass shaded off by gradations of tint answers well for this purpose.

though the stars under observation (or one of them) are too faint to permit of continuous vision. Bright wire illumination on a dark field is very useful in such cases, but this may be beyond the reach of many amateurs.

A second pair of distance "wires" of thicker web will often be found very useful when faint stars have to be measured, and the illumination of the field has to be kept down so low as to render the ordinary webs invisible. For these thicker "wires" the guy lines supporting a web may be used. They are of about the thickness of a fibre of silk and consist of many ordinary lines of web made by the spider into what may be called a cable.

It will occasionally happen that the "wires" (spider webs) of a micrometer break, as the result, it may be, of lack of care in handling the instrument, or even owing to the weather. This may be a source of inconvenience of a serious character to an observer if he is living at a place remote from an optician. A thick metal wire is renewable without much difficulty by anybody possessed of a moderate amount of mechanical skill, but to replace a genuine spider line may be a matter of some difficulty, first of all to obtain such a thing, and then to fix it.

In order to obtain a supply of spider line take a piece of moderately stout iron or copper wire about 9 inches in length and of such thickness (say 22 Birmingham wire-gauge) as to be fairly rigid when so bent into the form of a 2-pronged fork that the ends may be about three inches apart. Next, hunt up a spider and persuade him to attach his web to the wire fork near one end and then cause him to drop a few inches and catch the web on the other prong of the fork; then by turning the fork round one can wind up as many turns of the web as one cares to have, taking care to keep each new turn separate from, that is, not in contact with, the previous one. If this part of the business has been satisfactorily accomplished a stock of spider line will have been laid up for use.

The next thing is to render the lines secure on the fork before selecting and removing one preparatory to transferring it to the micrometer. This is done by touching both ends of each turn of the coil where it passes round the fork with a drop of shellac varnish or other sticky stuff. When this has set and is dry, the removal of any one web may be accomplished without affecting the others.

When it is desired to take a web out of stock in order to use it, make a second stout wire fork, rather less in breadth between the prongs than the first one. Put some sticky stuff on the tip of each prong and lift off a length of line from the storage fork. Remove the cover of the micrometer; clean off any old ends of broken web; touch with varnish each spot where the ends of the new web are wanted to go, and drop the new web into its proper place; let the varnish dry; and when the web is found to be in the right place and secure, cut off any overplus with a small pair of scissors. This final action in the work of renewal is more easily described than carried out, but with a little patience and the spoiling of a few webs, a final good result may reasonably be expected.

If you cannot secure the coöperation of a living spider to provide a new web, and have none in stock, search must be made for an old web *in situ*, and a suitable portion picked out and removed with a forked wire (such as already described) and baited, as it were, with some sticky stuff. A web thus casually obtained may probably be dusty, and may perhaps consist of more than one single strand, in which case certain obvious precautions must be taken and things done.

It will seldom happen that marks cannot be detected on the sliding fork of the micrometer to indicate where old webs have been attached, and therefore, where new ones must be applied so that they may be parallel to one another. Use a screw-driver which fits properly the small screws of the micrometer when it has to be taken apart. A sharp bradawl is a good substitute. Nothing looks worse than the head of a screw damaged by a tool unfit for work. While the screws are out, put them in a little box for safe keeping, for small micrometer (and other) screws have great straying power.

SPECULATIONS.

IN the volume of which I have made so much use, Innes has entered upon some reflections and speculations which, as they are not of the wild and irresponsible character so common now-a-days, I gladly quote by way of conclusion to this preliminary dissertation because of their present interest and prospective usefulness. He says:—

"The study of Double Stars will always be interesting from the Newtonian point of view, and in the case of the brighter pairs, the spectroscopic determination of motion in the line of sight will lead to a knowledge of the true position in space as well as of the absolute dimensions of their orbits, and hence their parallaxes. The irregularities of their orbital motions (already ascertained in a few cases, which will multiply as observations grow more precise) indicate the presence of disturbing bodies. For these reasons even the Binaries with the best-determined orbits deserve careful and regular observation. From the astro-physical point of view, it is hoped that much may be learned from their study. A contracting and rotating mass of gas may eventually reach an epoch when it will become unstable, and separate into two or more portions forming a Binary system. It is evident that this critical period will depend on the constitution of the body, its speed of rotation, mass and temperature. Systematic observations, micrometric and spectroscopic, should in time enlighten us on these, at present, obscure subjects. When we are able to arrange the elements of the orbits of say one thousand Binary pairs in tabular form, with their

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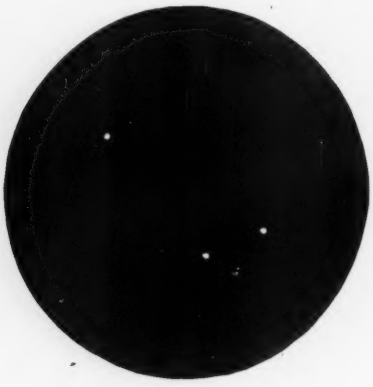
spectroscopic history, both of constitution and of radial speed, including for each the proper motion and its solar component, we shall be in a position to attack questions relating to the constitution of the sidereal heavens, which are to-day beyond the realms of reasonable speculation. In this respect the stars of the Southern hemisphere offer at least as attractive a field of investigation as those North of the equator. In spite of neglect, the Southern systems already exceed in importance those of the North. Among them may be quoted:—

Sirius	ω^2 Eridani
α Centauri	α Crucis
γ Virginis	β Muscae
γ Lupi	4 Aquarii
γ Centauri	ξ Scorpii
γ Circini	τ Lupi
γ Sextantis	β Phoenicis
ζ Aquarii	ζ Sagittarii

"It is true that of these α Crucis is not a Binary. It is perhaps all the more remarkable for that reason, being one of the brightest stars in the sky, and shown by photography to be surrounded by a cluster, or rather by rings, of very faint stars, in the centre of which the two large stars are, as far as can be ascertained, absolutely stationary. Are these gloriously brilliant stars devoid of mass, or, if they shine with an intrinsic brilliancy of surface equal to that of Sirius, at what distance must they be apart that gravitational motion becomes too small to be detected even in a century? Speculation and analogy alike fail us, and a systematic study of all Double Stars is alone likely to solve these and many other allied questions. The high eccentricity of the orbits of some Double Stars, including α Centauri, subjects the internal constitution of the components to different pressures at the times of peri-astron and ap-astron. Does this affect their spectra in a measurable degree?"*



Σ 2703 Delphini.

Mags. $7\frac{1}{2}$, $7\frac{1}{2}$, $7\frac{1}{2}$: Distances 25″, 71″, 59″.

γ Delphini.

Mags. $4\frac{1}{2}$, $5\frac{1}{2}$: Distance 11″.

β Cephei.

Mags. $3\frac{1}{2}$, 8: Distance 13″.

Cephei.

Mags. $5\frac{1}{2}$, $8\frac{1}{2}$, $8\frac{1}{2}$, 10: Distances $1\frac{3}{4}$″, 12″, 20″.

* Reference Catalogue of Southern Double Stars, page xiii *a*.

NERVES AND NERVOUSNESS.

By DAVID FRASER HARRIS, M.D., B.Sc. (LOND.)

(Lecturer on Physiology, University of Birmingham.)

POSSIBLY no terms are so loosely used as "nerves," and "nervousness." Owing to the prevailing ignorance about the nervous system they have become of much profit to the quack and the charlatan. Before we attempt to understand in what "nervousness" consists, and why some people are "nervous" and others not, we should find out what a nervous system does, what the possession of it means, and what would be the result of our not having one. The central nervous system is a mass of very highly specialised living matter (nerve-cells) contained inside the skull and spinal column in order to be protected from injury from outside. Into and out of this mass of nerve-cells run certain nerves from and to the "periphery," by which we mean all the body exclusive of the central nervous system itself. Most of the nerves going in, carry impulses which arouse sensations, most of the nerves going out, carry out impulses which arouse movement; by the former the environment acts on us, by the latter we act on the environment. The nervous system as a whole is *the* great means of communication between one part of the body and another; without it we should be totally unaware of what was going on around us. We should have no sensations and no pain; although possessing eyes we should not see, though having ears we should not hear; and, not having any sensations, we should have no emotions and no ideas, since these are higher mental states compounded of the more fundamental states of sensation and perception. Of course we should have no memory, as there would be nothing to remember. Again, if we had no nervous system, we should not be able to move a muscle when we wished; no matter in what danger we found ourselves we should be powerless to escape from it. One part of the body would not have the least idea where the other part was, what it was doing, or what it wanted: we should have no knowledge of the world around us or of our relationship to it. We could initiate no bodily activity, and so would be powerless to make any change whatever in the relations between the environment and ourselves. But by means of the afferent nerves the centres do receive information regarding both the body and the outside world, and, in consequence, the individual, through his centres and efferent nerves, can constantly adjust his body to the changing conditions of the environment.

The management of an army is a good analogy with the working of the nervous system. The army council—a few men—we may liken to the highest parts of the brain, the intelligence department and signallers to the afferent nerves, the rank and file of the soldiers to the muscles—the ultimate executants of the orders issued by the council. Now the soldiers, left to themselves, would never of themselves engage in any plan of concerted action. They must be drilled, made to execute first independent and then corporate movements in accordance with definite orders, the meaning of which they have previously learned. The men must be arranged in squads, companies, regiments and battalions, and must go through manoeuvres from time to time to practise what would be required of them in actual warfare. But in order that the army council issue appropriate commands, it must be kept informed as to the condition, number and distribution of all the units constituting the army. The soldiers are the muscles: if left to themselves, that is not attended to by the central nervous system, they might act spontaneously from time to time, but not always in a manner calculated to promote the well-being of the organism as a whole of which they form the constituent parts. The men must be drilled by sergeants who take orders from junior officers, who obey superior officers, who in turn obey orders ultimately emanating from the council at the War Office. Indirectly, then, this council issues orders to each individual soldier in the army. Similarly the brain is in touch with each muscle, which it also drills and exercises and keeps in readiness for future activity, a state we call "tone." The muscles, if not in constant functional connection with the nerve centres through the efferent nerves, would become toneless, slack, unready to contract when a motor impulse (command to action) arrived. But the very opposite is what we find: muscles duly innervated have a certain degree of tension, are ready to shorten after only an exceedingly brief time from the instant of receiving the message. Muscles not thus innervated, even though well supplied with blood would not be in a perfect state of health, would become "a law unto themselves," and therefore be unrelated to their neighbours' needs. This unfailing outflow of impulses from the cells of the central nervous system to the periphery—to muscles, blood-vessels, glands and possibly other tissues—is known as *innervation*. Innervation really means being attended to by the central nervous system; it is not being given strength to do work, but it is being kept in readiness to do work—a very different thing. It is being neurally supervised. Innervation is not the commissariat: the blood supplies the food; the blood is the canteen; each muscle must absorb its own nourishment, but by means of innervation it will be constantly kept "up to the mark," drilled, made tonic and ready for action. Innervation is each soldier's knowing that those in authority over him

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have not forgotten about him, that messages have been duly sent out to order his food to be brought into camp, that orders have been received as to how each minute of his day is to be occupied, and so on. A command to action imparts no strength to act, but it constitutes a necessary antecedent condition for an attentive, well drilled soldier to obey on the shortest notice in the best possible manner. This is what tone does for muscles; it keeps them ready. This outflow of what we may conveniently call tonic impulses has nothing to do with our consciousness; although diminished in intensity, for instance, during sleep, it is not abolished. We do not therefore consciously or voluntarily innervate our muscles: they are innervated subconsciously. When the nervous system dies the muscles take on the flaccidity of death before they enter upon *rigor mortis*, the rigidity of death.

Now we have a good deal of evidence that the outflow of these tonic impulses is rhythmic or intermittent, in other words at a certain regular rate per second. Physiologists are not agreed as to the exact rate of arrival of these at the muscle, but efforts are being made to determine it. In what we call a voluntary muscular contraction it is extremely probable that, although the intensity of these impulses is very greatly increased, their number per second, or periodicity, remains the same. The will, then, only exaggerates the existing state of tone; this exaggerated tone is a voluntary contraction, and hence it is sometimes said that "tone is incipient contraction"—a very good description. We know that heat is given out in voluntary contraction, but heat is also produced by a muscle in tone; and in proportion as the tone dies down so is the output of heat diminished.

This unconsciously exerted tonic influence of the nervous centres on the muscles is, like many other things, best realised when it is temporarily diminished or done away with. Thus, when a man gets a blow on the head, is "stunned," or suffers a severe and especially sudden injury, or has his central nervous system badly poisoned by alcohol or chloroform, he is quite unable to remain in a standing posture. This shock, or collapse, is due to his muscles having become more or less toneless, not so much because they are poisoned as because their innervation is reduced or abolished through the mechanical or chemical damage to the centres emitting the tone-maintaining impulses. The nerve-cells responsible for sending out these tone-preserving impulses are sometimes called "trophic centres," or centres related to trophism, a Greek word meaning "growth" but now taken as synonymous with tissue-health. Trophic nerves for muscles are none other than the efferent nerves conveying impulses inducing tone in the muscles. Trophic impulses in any other sense, in so far as muscles are concerned, do not exist.

All tissues, even such an apparently lifeless one as bone (which, however, is very much alive), when from any cause deprived of their nerve-supply suffer in health, become atrophic, or atrophy. It is equally correct to speak of blood-vessels and glands as being kept in tone or in a good trophic state by reason of their innervation. Thus, if the efferent nerve to any tissue is cut, the tone of that tissue is diminished or abolished for the time being, muscles become flabby, blood-vessels paralysed or dilated, and glands quite unhealthy. It will be convenient to have a term for the nerve-cell in the centre and its outgrowth, the efferent nerve-fibre, which passes all the way from the central nervous centre to the periphery; it is "the efferent neurone." Neurone means nerve-cell and all its processes including the long conducting process to the tissue or organ innervated. The nerve-fibres which stretch from nerve-cells to tissues have at least the property of conductivity, the power of conducting nerve-impulses. Toneless muscles are a sign of deficient innervation, a condition seen for instance typically in melancholia. It is, of course, a mental condition, but its outward and visible sign is deficiency of tone of all tissues not only muscles but blood-vessels, glands, and skin as well. Deficient innervation leads to deficient tone, whether that deficient innervation is due to mechanical injury to the nerve or to a depression of the emitting centre itself. In neurasthenia, again, innervation is deficient; not only are the muscles lacking in tone, but the glands, for instance the gastric glands, are deficient in chemical tone, and as they do not secrete sufficient hydrochloric acid there results the consequent nervous dyspepsia. Popularly a neurasthenic is "a nervous person"; strictly speaking such a person is suffering from weakness of the nerve-centres, probably due to their being poisoned or not sufficiently nourished at some previous time. Nerve-centres themselves, then, can be in good health, full of vigour and nerve-energy, or they can be less vigorous, putting forth less energy, be definitely weak. Neurasthenia is only Greek for *not-strength* of nerves; but in this case "nerves" means nerve-centres, since nerve-trunks are not sources of energy and merely conduct impulses. Nerve-energy or nervous energy are terms one reads about a great deal, sees very often used in advertisements of quack medicines, and so on; but it is a thing of which the man of science has very little knowledge. There must, of course, be such a thing as nerve-energy; else nerve-centres could effect nothing and affect nobody, for "*ex nihilo nihil fit*." The English physiological psychologist, Dr. MacDougal of Oxford, has coined the word "neurine" as a convenient term for nerve-force. Although we know almost nothing about nerve-force we know a little of what it can do, just as, although we do not know much about electricity, we know something of what it can do: both electricity and neurine pass from places of high to places of lower pressure. Returning to the centres in the spinal cord, we see that they are the sources of the nerve-energy sent out to the tissues everywhere; but we might at this point ask ourselves: do we know anything of the material basis of this nerve-energy?

(To be continued).

NOTES.

ASTRONOMY.

By F. A. BELLAMY, M.A., F.R.A.S.

FAYE'S COMET (or 1910 *e* Cerulli).—The comet announced as a discovery by Cerulli at Teramo Observatory, on November 8th, by means of photography, proves to be a re-discovery of the unexpected, or forgotten, return of the comet discovered by Faye in 1843. This comet is interesting as having a short periodic orbit of seven and a half years, or slightly less; so that, since 1843, this is the ninth return, and it happens to be a favourable one; it is not always so, as at some returns it could not be observed, notably in 1903. When discovered on November 8th it was of the 9·5 magnitude. After a few observations had been made, it was ascertained by W. F. Meyer and Miss S. H. Levy, of the Berkeley Astronomical Department, U.S.A., that the elements of the orbit which they computed resembled those of Faye's Comet, and they afterwards computed elliptic elements from observations of November 9th, 11th, and 13th; they give the date of perihelion passage as 1910, November 12·413, G.M.T., but M. Ebell (*Ast. Nach.* 4456), deducing the time from Professor Strömgren's elements for 1903, finds the date to be October 23·747 Berlin M.T. In *Ast. Nach.* 4461, G. Fayet has discussed the question both with new observations and old elements, and he obtains November 2·283 Paris M.T. as the date of passage, and the other elements are very close to those which he deduces from Strömgren's work. Fayet's elements are:—

T	=	1910 Nov. 2·28269. Paris M.T.
υ	=	199° 51′ 6″·74
Ω	=	206° 7′ 0″·31
ι	=	10° 30′ 24″·79
e	=	0·560806
log. a	=	0·575109
μ	=	486″·792

They should, however, be regarded only as provisional. Plenty of observations are being obtained and they will prove of use in the computation of a definitive orbit.

Fayet has also deduced an ephemeris for Paris mean midnight; from a portion given here it can be seen that the comet is now moving northwards and eastwards; it reached its most southerly declination on December 19th and 20th, and at the end of this month it is about the same right ascension as the Pleiades and twenty degrees south, and its magnitude will be equal to half that at the time of re-discovery.

1910–1911.	R.A. h.	m.	s.	Dec.	
Dec. 17 ...	3	39	13 ...	+3°	9″·5
,, 20 ...		40	23 ...	+3°	8″·5
,, 25 ...		42	51 ...	+3°	13′·9
,, 30 ...		45	58 ...	+3°	27″·2
Jan. 4 ...		49	45 ...	+3°	47″·2
,, 9 ...		54	11 ...	+4°	12″·6
,, 14 ...	3	59	10 ...	+4°	42″·3
,, 19 ...	4	4	43 ...	+5°	15′·3
,, 24 ...	4	10	43 ...	+5°	50″·7
,, 29 ...	4	17	12 ...	+6°	27′·6

HALLEY'S COMET.—It is still possible for observations to be obtained of this much-discussed comet. Those who wish to see it again before 1985, should not lose the opportunity during the next few weeks; it is best situated for observation about 5 a.m., and will be at a similar position about two and a half hours earlier each month; being at rather a low altitude it may be seen more readily at places south of England. This ephemeris is that given by F. E. Seagrave, in the *Astronomical Journal*, No. 620.

Midnight G.M.T.

1910.	h.	m.	s.		
Jan. 3 ...	11	51	27	...	−18° 18″·1
,, 11 ...		44	53	...	−18° 26″·1
,, 19 ...		36	57	...	−18° 23″·9
,, 27 ...		27	51	...	−18° 10″·8
Feb. 4 ...		17	44	...	−17° 45″·6
,, 12 ...	11	6	56	...	−17° 8″·1
,, 20 ...	10	55	49	...	−16° 19″·2
,, 28 ...		44	44	...	−15° 20″·5
Mar. 8 ...		34	6	...	−14° 14″·3
,, 16 ...		24	15	...	−13° 3″·7
,, 24 ...		15	25	...	−11° 51″·6
April 1 ...	10	7	49	...	−10 40′·7

ASTRONOMER ROYAL FOR SCOTLAND.—We offer our congratulations to Professor R. A. Sampson upon his appointment to the position of Astronomer Royal for Scotland. His works upon J. C. Adams and Jupiter's Satellites have hitherto absorbed nearly all his time given to astronomical work; both these memoirs are of great and lasting importance. Now that he has charge of an observatory worthy of the name and one with an excellent and efficient all-round equipment, we look forward to a continuance and development of that practical side of astronomy so ably inaugurated at Edinburgh by the newly-appointed Astronomer Royal for England, Mr. F. W. Dyson.

THE PRESENT STATE OF VARIABLE STAR WORK. INTRODUCTION.—Exclusivism seems to be the worst policy in scientific research. As observational science is now highly developed in practically all the countries of the world, and all the workers are exploring almost the same borderland of discovery, accurate and ever-renewed knowledge of what others have done and are doing, is an absolute requirement if unnecessary duplication is to be avoided, as well as the waste of energy, time and money. This knowledge, however, is not always as perfect as it ought to be, especially in great countries such as Britain, France, and the United States, where scientific men, less inclined to internationalism and to the study of foreign languages, are sometimes tempted to consider the scientific activity of their countrymen as representing science. Small countries are better able to avoid this drawback, for, lacking elaborate sources of scientific information, forced to be polyglots and naturally exempted from any undue chauvinism, their men of science are in a better position to consider from an unbiassed point of view the efforts of the whole of the scientific world. And this, perhaps, is one of the reasons why countries, small in their area and economic possibilities, maintain a high standard of research among their powerful rivals.

In the peculiar field of astronomy in which I have been engaged for many years—the study of those interesting objects, variable stars—much correspondence with British amateurs has shown me how often their views on this subject are confined to English references. I therefore think that a comprehensive statement of what is actually done in this branch, in the whole world, may be welcomed by all interested in the matter including those possessing a telescope, whatever its size may be. It will not only give them an idea of the work of astronomers in this direction, in different countries, but will also show them how the observation of changing stars is carried out with zeal and success by isolated workers.

STATISTICS OF DISCOVERY.—Many complaints have been made recently with regard to the great disproportion which exists between the great number of variable stars known

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at the present day and the attention shown to them. A mere glance at a catalogue of such objects will make this perfectly clear and illustrate how many, and even essential facts, remain unknown; especially as to long period variables, which should all be followed in a permanent manner in view of a better knowledge of their light-curves.

In order to bring this out more clearly, I have drawn up a table of the discovery of variable stars during recent years. For this, I deduced from a manuscript catalogue of such objects, for each year during the last decade (1900–1909), the number of variables announced as being variable, and to which a "provisional number" has been affixed, together with the number of the same stars that have received up to the present a "final name"—a combination of two letters—as explained hereafter, and the proportion between these two series of figures.

Variables in clusters have not been included in the lists, but *novae* or temporary stars have been, since they are in the A.G. lists they were considered as lettered. The discovery at Harvard and at Heidelberg of a large number of very faint variables accounts for such great totals as that of 1904, for example, and I intended at first to exclude them from the statistics; but, as the variability of several of these objects has been confirmed and as they thus received final names, I ultimately included them.

In dealing with the total number of "provisional variables" care must be taken to exclude some "apparent" variables. For instance 19.1900 Puppis is a rediscovery of *Z* Puppis; 67.1901 and 74.1901 are ρ Cassiopeiae and κ Persei respectively, and as they possess Bayer (Greek) letters, they will never receive final names in the usual nomenclature, and have been excluded; 2.1902 is a rediscovery of U Lacertae; 9.1902 and 43.1909 are the result of mistakes and never had a real existence.

In the following table, the first column contains the total (apparent) number of provisional numbers of variables as shown by the *Astronomische Nachrichten*; the second the true or real number of discoveries; the third, the number of variables which have been lettered up to the present (including the latest list of the A.G. Committee in *A.N.* 4364); the fourth, the number of variables still unlettered and of which, therefore, little or nothing is known; the fifth and final column, the proportion between the real number of discoveries and the lettered variables. The small number of lettered variables in 1909 is of course partly due to the fact that they had only been known a short time.

Year.	(1) Apparent Number.	(2) Real Number.	(3) Lettered.	(4) Non-Lettered.	(5) Proportion.
1900	29	28	28	0	100·0 %
1901	99	97	61	36	62·9
1902	23	21	21	0	100·0
1903	86	86	43	43	50·0
1904	283	283	60	223	21·2
1905	171	171	38	133	22·2
1906	194	194	47	147	24·2
1907	223	223	101	122	45·3
1908	175	175	35	140	20·0
1909	46	45	10	35	22·2
1900-09	1329	1323	444	879	33·6 %

It will be seen that in recent years the proportion between new variables and those that are more or less "settled" is a little more than 20%, and that 66% of all the variable stars discovered during the last decade still awaits confirmation. This fact will appear more strikingly still on the inspection of Figure 1, where they have been graphically represented.

If British amateurs will consider that their own country offers them, through the medium of the "Variable Star Section" of the "British Astronomical Association," so ably conducted by Mr. C. L. Brook, F.R.A.S., an amateur himself, a splendid and almost unique opportunity to achieve, with the most simple appliances, a mass of useful work, and to contribute

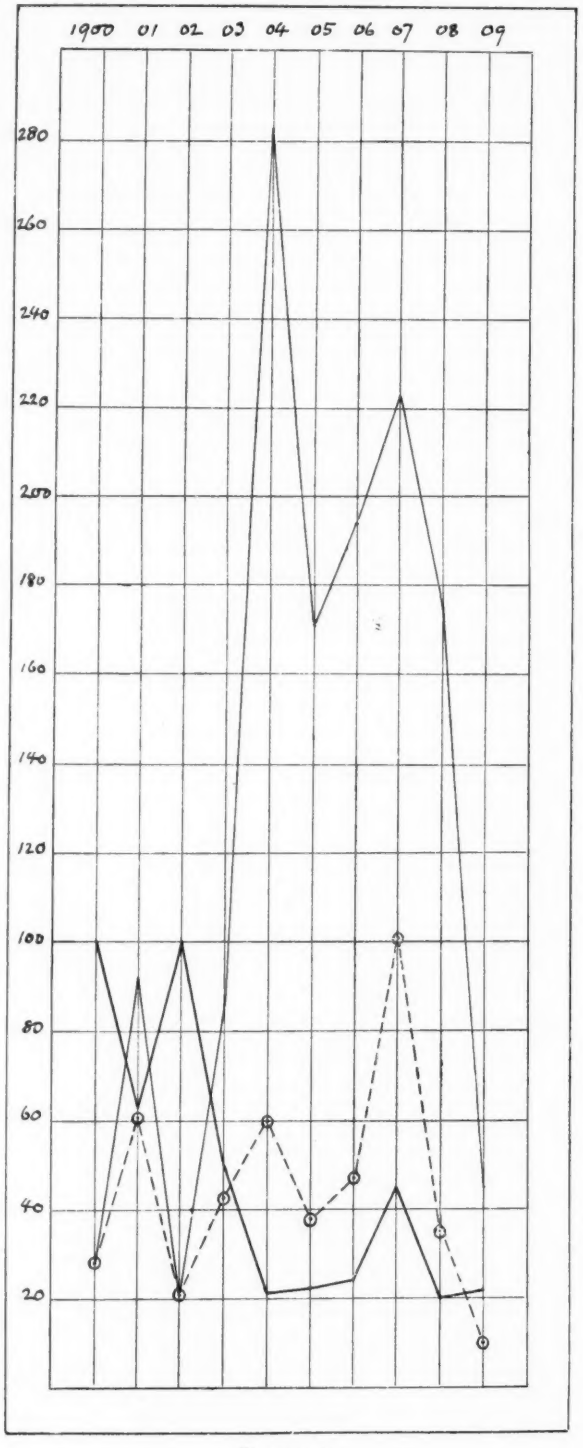

FIGURE 1.

The Discovery of Variable Stars.

The thin line represents the number of variable stars discovered each year since 1900; the interrupted line the number of the same stars whose variability has been confirmed; and the heavy line at the base, the proportion between both.

to our knowledge of the mysterious laws which govern the sidereal universe, I hope that some of them at least will seize this opportunity at once, and join the small band of observers, to which I am proud to belong, whose scientific activity is greatly appreciated; if not very fully in the British Isles, at least elsewhere.

FELIX DE ROY,

Honorary Secretary "Société d'Astronomie d'Anvers."

(To be continued.)

BOTANY.

By PROFESSOR F. CAVERS, D.Sc.

SUSPENSOR IN FERN EMBRYOS.—It was recently shown by Lyon that the embryo of *Botrychium obliquum* has a suspensor, while Campbell recorded a similar structure in the case of *Danaea*. The interest of these discoveries lies in the fact that hitherto a suspensor has only been known to occur in the embryo of *Lycopodium* and *Selaginella* among the Fern Alliance, or Pteridophyta. Lang (*Ann. Bot.*, 1910) has just discovered that *Helminthostachys*, which is closely allied to *Botrychium*, has a massive suspensor like that of the latter genus.

THE ECOLOGY OF CONIFERS.—The ecological features of the Coniferae are discussed by Groom (*Ann. Bot.*, 1910), who sets out various problems relating to this interesting subject. The main questions are: (1) the cause of the xerophytic foliage and tracheïdal wood; (2) the cause of the survival of Conifers in competition with Dicotyledonous trees; (3) the cause of the suppression of various Conifers in past ages. Stress is laid on the fact that all Conifers are not xerophytic, in spite of their xerophytic leaf structure. Höhnel showed that the Larch has a rapid transpiration current, while Groom's experiments show that Coniferous wood, in spite of its tracheidal structure, may conduct water with a rapidity equal to that of a rapidly transpiring Dicotyledonous tree. Another point is noted in this connection—the aggregate leaf surface of a Coniferous tree may exceed that of a Dicotyledonous tree, because of the immense number of the leaves. This leads to the view that the xerophytic structure of the Conifer leaf is actually necessary because of the great amount of exposed surface, and the term "architectural xerophytism" is suggested for xerophytism that is dependent, as in this case, upon the organization of the plant rather than upon the direct influence of external factors on the organs in question. The extinction of many of the ancient Conifers is attributed to their imperfect acclimatization, to the fact that they have a large number of insect and fungus enemies, and to their relatively slight power to react advantageously to new conditions. At the same time, their "architectural" xerophytism enables them to thrive in nearly all situations, from those that are physically or physiologically dry to those that are sufficiently humid to permit the development of luxuriant non-xerophytic forests.

"GERMANOL."—A Berlin syndicate has placed on the market a product called "Germanol," consisting of an earthy mixture containing about eighteen per cent. of calcinated soda. The virtue of this mixture, which is intended for agricultural use, is attributed by the manufacturers to an increased porosity of the soil following an increase in the proportion of carbon dioxide, the latter being supposed to act as a "fertiliser." Mitscherlich (*Landwirtsch. Jahrb.*, 1910) has made extensive experiments, however, which appear to show that if this mixture has any value at all it must be attributed to the action of the carbon dioxide in increasing the solubility of difficultly soluble substances in the soil. He concludes, moreover, that increasing the carbon dioxide content of the soil does *not* result in an increase of plant product; that there is always sufficient carbon dioxide in the soil already to render mineral food available; and that any increase in the solubility of soil constituents due to increase on carbon dioxide content is absolutely superfluous so far as any advantage to the plant is concerned. It would seem, therefore, that "Germanol" will prove a failure in practice, just as various other nostrums, like "Nitragin" and "Nitro-bacterine," have done in the past.

BOTANY IN CALIFORNIA.—Some recent publications on Botany issued by the University of California are of general interest. They include a beautifully illustrated paper by Hall on ornamental trees, especially the "bottle-brush" genera of the Eucalyptus order (Myrtaceae). Gardner describes a new genus of Flagellates (*Leuvenia*) which is of special interest in view of the importance attached to these lowly organisms lying at the common base of the animal and vegetable series. This new genus presents a remarkable combination of characters which make it difficult "at present to classify it, even to naming the family to which it belongs or in which it has its nearest affinities." The nuclei and pigment-bodies (chromatophores) are inconstant in number, while in the motile phases contractile vacuoles occur at both the anterior and posterior ends, while pyrenoids, gullet, and eye-spot are absent. The same author also describes two new genera of Green Algae which grow in association with other marine algae of California. *Endophyton* grows inside the frond of Red Algae of various kinds, and apparently forms a link between the families Chroolepideae and Chaetophoreae, differing from the latter in the absence of hairs. *Pseudodictyon*, referred to Chroolepideae, grows among the cells of the cortex in species of tangle (*Laminaria*).

CHEMISTRY.

By C. AINSWORTH MITCHELL, B.A. (Oxon.), F.I.C.

ACTION OF POISONOUS ALKALOIDS ON PLANTS.—An instructive series of experiments has been carried out by Messrs. Otto and Kooper (*Landw. Jahrb.*, 1910, xxxix, 397) to ascertain the effect of adding dilute solutions of poisonous alkaloids to the soil in which plants are grown. It was found that when a relatively small area of soil was treated daily with a solution of strychnine sulphate, the liquid first filtering through the soil contained the acid constituent of the salt, but not the alkaloid, though this, too, eventually passed through the soil unchanged. This phenomenon of temporary absorption of the alkaloid was also observed in the case of perfectly sterilised soil, and was thus not due to bacterial action. The presence of solutions of salts of alkaloids, such as strychnine, in the soil had a pronounced effect upon plants growing therein, checking both germination and growth, and causing the fruit to be abnormal. Nicotine added in the form of a three per cent. solution was retained by the soil, apparently through the formation of a loose "addition compound," for the alkaloid was not altered chemically by the absorption. Eventually a portion of the nicotine was decomposed and volatilised, this being apparently brought about largely by the influence of heat and moisture in the soil. The growth of the tobacco plant was greatly stimulated by the addition of nicotine solution to the soil, and the proportion of nicotine in the plant showed an increase. The alkaloid had a similar, though less pronounced, effect upon the growth of the potato, and did not appear to affect the composition of the plant. Treatment of the soil with a solution of sodium nitrate had a similar stimulative action upon the growth of the tobacco plant.

THE GLAZING OF ANCIENT GREEK VASES.—The nature of the black glaze upon a Greek vase excavated at the Heraeum has been examined by Mr. W. Foster (*Journ. Amer. Chem. Soc.*, 1910, xxxii, 1259), who has found that it does not contain manganese, but consists of an iron compound, probably ferrous silicate. The red glaze upon a Mycenian vase has also been examined, the surface of the vase being scraped with a diamond, and an analysis made of the resulting powder. The conclusion drawn from the results is that the colour of the glazing is due to the presence of a ferric compound.

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Analyses of the body of Mycenian vases showed that they contained from approximately 41 to 48 per cent. of silica, 17 to 20 per cent. of alumina, 7 to 9 per cent. of iron oxide, 14 to 20 per cent. of lime, 4·5 per cent. of magnesia, about 3 per cent. of potassium oxide and about 5·5 per cent. of carbonate. From these analyses it appears that Mycenian pottery differs from Attic and Campanian pottery in containing much less silica but much more lime, and in the relatively large proportion of carbonate present. Owing to its low proportion in silica the pottery can be readily melted at a relatively low temperature.

THE DIGESTIBILITY OF BLEACHED FLOUR.—Of the numerous methods that have been proposed for bleaching flour, only those in which nitrogen peroxide is employed as the bleaching agent have proved successful upon a commercial scale. Flour bleached in this manner is now largely sold, and on more than one occasion has been the subject of actions in the law courts. Hence the experiments of Mr. E. W. Rockwood upon the effect of bleaching upon the digestibility of flour (*Journ. Biol. Chem.*, 1910, viii, 327) have a special interest at the present time. In these experiments the tests were applied simultaneously to bleached and unbleached samples of the same flour, the conditions in each case being strictly parallel. It was found that uncooked gluten separated from the flours was digested by pepsin with equal rapidity in each instance, while the product from the bleached flour was digested more readily by pancreatin than that from the unbleached specimen. The cooked gluten from the bleached flour was more easily digested both by pepsin and pancreatin than that from unbleached flour, while bread made from the two kinds of flour did not differ in its digestibility, the nitrous compound having been expelled during the baking. The bleaching process was also found to have had no appreciable influence upon the digestibility of the starch in the flour.

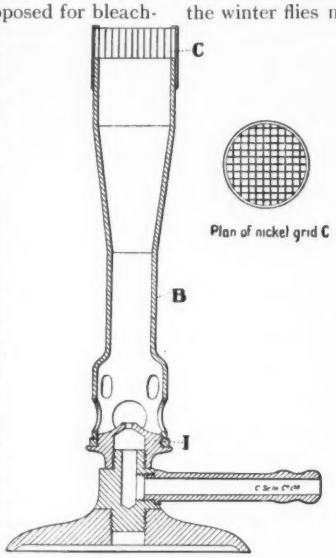

FIGURE 1.
The Méker Burner.

THE MÉKER BURNER.—A new gas burner, presenting many advantages over the ordinary Bunsen burner, has been put upon the market by the Cambridge Scientific Instrument Company, under the name of the Méker burner. The construction of this is shown in the accompanying figure, from which it will be seen that the holes for the admission of air are much larger than in the Bunsen burner, so that the mixture of air and gas in the chimney is of the right proportion for complete combustion. A deep grid of nickel is screwed to the top of the burner to prevent the flame "striking back." The flame of this burner is homogeneous, and does not show the inner and outer cone of the Bunsen flame. Its temperature is much higher than that of the latter, and is nearly uniform throughout. In fact, the coolest part of a Méker flame is hotter than the hottest portion of a Bunsen flame. A burner of modified form has been made for use with compressed air, thus enabling a yet higher temperature to be reached.

ECONOMIC BIOLOGY.

By WALTER E. COLLINGE, M.Sc., F.L.S., F.E.S.

HOUSE FLIES AND DISEASE.—I promised last month to deal with some of the correspondence I have received with reference to house flies as transmitters of disease. A renewed interest, I am pleased to say, has been awakened in the subject, and, as I recently stated, I trust the public will ultimately educate the authorities in order that our different public bodies will realize the seriousness of the matter. No one wishes to create a scare, or to indulge in wild unguarded statements, but to calmly and judicially consider the facts as brought out by recent investigations.

FLIES IN WINTER.—One correspondent inquires what becomes of the flies in winter,—do they die, leaving eggs to hatch later, or do they hibernate? Mr. F. P. Jepson has shown (*Journ. Econ. Biol.*, 1909, vol. iv.) by marking flies in the summer of 1908, for purposes of identification, that nearly all of them were captured in a bakehouse, the supply continuing up to nearly the middle of November, when a sudden sharp frost seemingly caused their sudden disappearance; they reappeared, however, on February 15th, 1909. From these and other experiments he concludes that during the winter flies may be found where the conditions—temperature and so on—are favourable, and that they will breed in the winter. Many undoubtedly persist throughout the winter as adults, and appear to be more hardy and longer lived than those taken in the summer.

THE SPREAD OF INFECTIVE DISEASES.—The belief that flies may carry the germs of different diseases dates back to the sixteenth century, but actual experiments proving this are of much more recent date. Experiment has now actually demonstrated that the following diseases can be disseminated by the agency of house-flies:—Anthrax, cholera, ophthalmia, tuberculosis and typhoid fever. In addition to these there are a number of diseases which are probably carried by flies, but the evidence, as yet, is inconclusive.

There is now evidence sufficiently clear to show that, apart from disseminating the germs of the above diseases, flies also carry the eggs of parasitic worms and various species of fungi.

Professor Nuttall very pertinently remarks (Local Government Board Report, No. 16, 1909):—"It should be remembered that a fly may cause relatively gross infection of any food upon which it alights after having fed upon infective substances, be they typhoid, cholera or diarrhoea stools. Not only is its exterior contaminated, but its intestine is charged with infective material in concentrated form which may be discharged undigested upon fresh food which it seeks. Consequently, the excrement voided by a single fly may contain a greater quantity of the infective agents than, for instance, a sample of infected water. In potential possibilities the droppings of one fly may, in certain circumstances, weigh in the balance as against buckets of water or of milk."

PROGRESS OF THE WORK.—Our grand-parents would have laughed at the idea of attempting to control such ubiquitous pests as house-flies; indeed, there are still people who are unconvinced as to the seriousness of the part these insects play in every-day life, as is instanced by many of the letters I have received during the past two months; but, thanks to the excellent work that is being carried out at Cambridge and elsewhere, it will not be long before the public are not only convinced, but they will make themselves heard for drastic alterations that will tend to minimise the danger.

Dr. Monckton Copeman has recently scheduled (Local Government Board Report, No. 40, 1910) the work that is being done at Cambridge, which includes special attention to the elucidation of the question as to the range of flight of flies, both in horizontal and vertical directions; and under varying conditions, meteorological and otherwise. Trials are to be made of the respective value of various baits for attracting and killing flies. The so-called lesser house-fly,

Homalomyia canicularis, is to be studied. Dr. Graham-Smith will continue his work on the experimental infection of flies with pathogenic micro-organisms, whilst Dr. Nicoll will inquire into the possible agency of flies as carriers of the eggs of parasitic worms, and so on.

These are a few of the lines of research and experimentation, all of which will increase our knowledge and place us in a more able position to effectively deal with a pest we can well afford to be without, and one in dealing with which the general public can materially assist.

GEOLOGY.

By RUSSELL F. GWINNELL, B.Sc., A.R.C.S., F.G.S.

WIND AND WATER.—Quite a number of papers published within the last few months deal with such subjects as the movements of wind and water, and the resulting aeolian and aqueous deposits.

In "The Origin and Growth of Ripple Marks" (*Proc. Roy. Soc. A.*, Vol. lxxxiv), Mrs. Ayrton describes experiments leading to the conclusion that there are three ways of originating sand ripples under water. Two of these methods are capable of originating and then extending regular ripple-marks wherever water is oscillating over sand, whether its surface be smooth or uneven to start with. These are:—

(1) The "Differential Motion" Method, by which single ridges arise at the loops of stationary waves.
(2) The "Brush" method, whereby the brushing action of the vortex in the lee of any existing dune or obstacle sweeps up a new ridge beside it, leaving a hollow along the neutral line of the vortex.

The third method, depending on an initial uneven surface, could, unaided, give rise to *irregular* ripple-mark only, since it consists in piling up sand in places where the surface chances to be uneven.

Summarily the processes by which mounds of sand are built up under the action of stationary waves are:—

(1) Ripples always first form at places of maximum longitudinal motion of the water, *i.e.*, at places of constant level.
(2) Smooth spaces are left at and near the places where the water has no longitudinal motion, *i.e.*, where the change of level is greatest.
(3) Each set of ripples grows into a mound having its centre plane at the place of maximum longitudinal velocity and its lowest parts close to places where the longitudinal motion is zero.
(4) There are therefore two of these mounds to each water wave.

Mr. A. Wade, B.Sc., F.G.S., in the *Geological Magazine* September, 1910, deals with "The Formation of Dreikante in Desert Regions." These curious three-edged stones, so characteristic and abundant in most desert regions, are shaped somewhat like a Brazil nut. The old explanation of their fluviatile origin has long been discarded in favour of an aeolian origin, which supposes that the wind (laden with sand and thus a powerful eroding agent) strikes the point of the pebble, glancing off on either side so that the "keel" on the upper surface of the pebble is in the direction of the wind. If this is the true explanation, it is difficult to make it tally with observations recently carried out by the author in the Egyptian Desert.

Thus he finds that the Dreikante are equally pointed at both ends; collecting over three hundred specimens in an area of about three hundred acres, he finds 78% set with the "keel" approximately at ***right angles*** to the wind; the keel instead of being a simple straight one is often curved or even bifurcating at both ends, giving a five-faced solid instead of a three-faced one; the faces are parabolic curves; finally, actual experiment proved that these three-edged stones could not remain with their pointed ends facing a strong current of wind.

The author, therefore, shows that the travelling sand-grains hit not edgewise but broadside on a face; and being projected off again in an upward direction, the parabolic curve is produced. The cutting-away continues until the pebble stands on a very narrow base, and then topples over; thus the erosion commences again on a fresh surface. "Sometimes at night the rattling of the pebbles moving under the action of a high wind is very considerable."

In the November issue of the same magazine is printed a summary of the views of the Russian geologist, Ivchenko, as to aeolian deposits. He classifies the various types of stratification in aeolian deposits (such as ripple-mark, diagonal stratification with opposed dips, horizontal, vertical, and so on), and their modes of origin. In addition to ordinary winds the action of whirlwinds can often be distinguished by the spiral or circular arrangement of the dust particles. The dimensions of particles of desert dust transported by the wind diminish from the centre towards the margins of the desert. The grains of sand have special characters in aeolian and in aquatic deposits; in the former they are characteristically triangular.

Finally, we may note two papers by Mr. A. R. Horwood, of Leicester Museum, on "The Origin of the British Trias." One paper was read before the British Association in September last, the other before the Geological Society on November 23rd. The general view of conditions in Triassic times is that the deposits appear, on the whole, to have been laid down under conditions similar to those which now prevail in the dry interior of continents. The lower-lying country between the mountain ranges was covered by wind-blown sand and alluvial deposits washed down from the hills during occasional floods, and gradually the mountain-chains were more or less completely buried. In hollows in the nearly level plains that were thus produced, the water collected and was evaporated, forming lakes of brine. (See Lake and Rastall: "Text Book of Geology," 1910).

Mr. Horwood, however, considers the British Trias to be a delta deposit, in which desert conditions did nothing more than locally act upon the rock. ***They had no part whatever in the work of deposition.*** Among many other points, he notes that ripple-mark affects only the sandstones, and not the marls; also that, whereas *all* varieties of desert sand agree in possessing the texture of miniature pebbles with a highly polished surface, all Trias sediments exhibit an eroded surface.

He maintains that there is positive, direct and accumulative evidence to prove that the Trias as a whole (and not the Bunter only) was the work of rivers which had continued to bring sediment in one form or another from the north-west of Britain more or less continuously from the close of the marine phase of Lower Carboniferous times.

THE ORIGIN OF CORAL ISLANDS.—Reef-building corals can only thrive in seawater which does not exceed about thirty fathoms in depth, and the temperature of which never falls below 20° C. (68° F). The latter limitation almost restricts coral islands to tropical seas; the former implies a shallow submarine foundation in every case. The explanation of the occurrence of such shallow foundations or submarine plateaux, far from continental land, has been more or less of a mystery right down to the present day. The fact of the existence of the plateaux is established, and also that the material is frequently volcanic; in other cases the living coral is founded on an enormous thickness of dead coral rock (as at Funafuti, where a boring of over a thousand feet encountered nothing but coral limestone).

The curious atoll-structure, where a ring of coral islets more or less encloses a central lagoon, renders the explanation more difficult. The main theories are those of Darwin, Dana, and others, on the one hand, and of Murray, Agassiz, on the other. Darwin's hypothesis necessitates subsidence of innumerable islands, the corals growing upwards on a talus-slope of their own debris as fast as the islands sink. This implies widespread crustal subsidence. In Murray's eyes the submarine platforms are volcanic islands denuded to a "plain of marine erosion," and the conditions are those of general elevation or long-continued crustal repose.

R. A. Daly, writing in the *American Journal of Science* (November, 1910) on Pleistocene Glaciation and the Coral

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Reef Problem, explains the phenomena of coral islands as being due neither to crustal subsidence nor to elevation of volcanic islands, but to *changes in the sea-level itself.* These changes, broadly treated, consisted in a lowering of the water-level, followed by a return to normal conditions as the great ice-caps first grew to enormous dimensions, and then retreated during the Great Ice Age. Estimating the Polar ice-caps during the glacial epoch at an area of six million square miles, with an average thickness of three thousand to five thousand feet, the author points out that the removal of this water would of itself lower the general sea-level by one hundred and twenty-five to two hundred and eight feet (say twenty to thirty-five fathoms). Besides this, however, the gravitational attraction of this great mass of ice would heap up the ocean water nearest it to such an extent as to lower the equatorial sea-level another five to eight fathoms. Thus the total lowering of sea-level in the tropics would be quite thirty fathoms. Conversely, the deglaciation of the whole ice-cap would *raise* it to this extent.

Now, the submarine plateaux on which coral reefs are found do not, as a rule, depart very much from an average of about thirty-five fathoms below present sea-level. Allowing for a "veneer" of coral detritus, etc., of ten to fifteen fathoms in thickness, the "solid rock" is about forty-five to fifty fathoms under water. The *uniformity* of the plateau level is explained by neither Darwin's nor Murray's hypothesis: the present author explains it as being due to marine abrasion during the Glacial Period. Allowing for the thirty fathom lowering of sea-level produced by the Polar accumulation of ice, this platform would then be within fifteen to twenty fathoms of the surface, a depth in which the *growth of the coral organism is possible.*

If the whole earth were chilled during the Glacial epoch (as supposed by some) reef-building corals would be vastly restricted in areal distribution. A fall of six degrees only would enormously diminish the area where the marine isotherms never fall below 20° C., the temperature necessary to coral life. This would mean that most of the present areas of coral seas were then free from such life. As the retreating waters caused low Pleistocene islands to emerge from the sea and no coral fringes protected the loose volcanic or calcareous material, denudation would be very rapid. Exposed on all sides to abrasion by the open sea, these islands would lose substance at least as fast as the chalk cliffs at Dover are doing (about three and a half feet per annum). At this rate the Chagos Bank, now a huge plateau, sixty miles broad by ninety miles long, could have been reduced to the Pleistocene sea-level in about fifty thousand years; and the whole duration of maximum glaciation in Pleistocene times was much longer than that. Moreover, in the case of many areas this Pleistocene denudation was only the "finishing touch" to subaerial denudation, which might have been continuously going on previously. Some of the volcanoes, indeed, may have been not merely pre-Tertiary, but even pre-Cambrian.

Amelioration of climate followed, allowing coral life in the waters where shallow platforms were now prepared for them.

At the same time deglaciation caused a gradual deepening of the sea. The coral growth would easily be fast enough to keep the living zone of coral within twenty fathoms of the surface during this deepening. The mechanism of growth, with the resulting development of atoll, barrier and fringing reef forms would be analogous to that imagined by Darwin on the subsidence hypothesis.

The theory of a general lowering of sea-level in inter-tropical seas—counteracted in higher latitudes by the glacial attraction—in no sense excludes complications due to local warpings of the earth's crust. Other processes invoked by previous writers have been so far neglected in the argument because of the high probability that neither basin-foundering, nor sedimentation, nor absorption of water by weathering rocks seem to have been important enough since Tertiary times to affect, by more than a fathom or two, the changes of level assumed.

METEOROLOGY.

By JOHN A. CURTIS, F.R.MET.SOC.

THE week ended November 19th, opened with extremely unsettled weather in all districts, with heavy rain, snow and sleet. Thunderstorms and hail were reported on several days. In the South, however, there was a decided improvement in the second half of the week. Temperature was below the average everywhere, by as much as 7° in places, while sunshine was, on the whole, in excess. In Ireland and in Scotland W., the amount of rainfall for the week was below the average, though the number of rainy days were in excess there as elsewhere. The mean temperature of the sea water was higher than that of the air on all our coasts, the excess at Plymouth being 10°. The highest temperature reported was 58° at Geldeston on the 15th, the lowest 18° at Marlborough on the 17th.

The week ended November 26th, was cold generally, but the weather was finer in the Southern and Eastern districts than in the Northern and Western. The defect in temperature exceeded 9° at several stations, and at Prestwich it reached 10°. At three stations in Ireland there was a slight excess. The highest temperature reported for the week, 59°, was in Ireland on the 23rd. The lowest minimum was 10° at Balmoral on the same day. In Westminster the highest reading was 43°, and the lowest 25°. The amount of rainfall varied much. In the English Channel it was three times the average, and in Ireland S. nearly twice as much, while in Scotland N. it was not quite half as much as usual. In the Eastern districts generally the rainfall was in defect, and at Spurn Head the week was rainless. Sunshine was in excess except in the Southern districts. Balruddery reported the highest aggregate, 22·7 hours or 43 %. The mean temperature of the sea water ranged from 53°·0 at Teelin to 40°·0 at Pennan Bay.

The week ended December 3rd, was, as a rule, dull and unsettled, with frequent and long-continued rains. In the extreme North, however, and in Ireland, the weather was bright and cold. Aurora was seen in Scotland on November 29th and 30th. Temperature was low everywhere, the defect exceeding 7° in places; the highest reported reading was 56° at Jersey on the 27th, the lowest 19°, at West Linton and Markree Castle on the 30th. Rainfall again varied greatly, it being only one-fifth of the average in Scotland N., while in the Midland Counties it was five times the average. Falls of 1-in. and upwards in twenty-four hours were frequent, and at Raunds in the four days, December 1–4, the total precipitation was 4·23-ins. Sunshine was in excess of the average in Scotland N., and in Ireland N., but in no other district. At Jersey the total duration for the week was only 5·8 hours (10 %), while at Castlebay it was 33·4 hours (67 %). In London the week was sunless. The mean temperature of the sea water was greater than that of the air on all our coasts. The individual readings varied from 53° at Plymouth, to 40° at Cromarty.

The week ended December 10th, was dull, mild and wet, with a good deal of strong wind. Temperature was high everywhere, the excess exceeding 6° at several places. The highest readings reported were 57° at Cirencester on the 7th, and at Llandudno on the 9th, the lowest, 29°, at Balmoral and at Arlington. Over a large part of the kingdom the temperature of the air did not fall below the freezing-point, and on the ground the minimum was only 25°, at Balmoral. The rainy days were in excess of the average, and at many stations rain was measured each day. The amount of rainfall was, however, not excessive, except in England S.W., where it was nearly twice as much as usual. In some districts, indeed, the rainfall amount was in defect, and in Scotland N. it was but little more than one-third of the average. Sunshine was scanty in all districts. At Westminster the total duration was only 1·3 hours (2 %) while the highest amount reported was only 13·5 hours (25 %) at Felixstowe. The temperature of the sea water varied from 52° at Plymouth to 39° at Cromarty.

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THE AUTUMN OF 1910.—For Meteorological purposes the Autumn is taken as the three months, September, October, November, and of the thirteen weeks, September 4th to December 3rd, 1910, in England S.E., four weeks have been unusually warm, eight weeks unusually cold and only one week about normal. During the same period five weeks have been classed as weeks of heavy rainfall, four as weeks of light rainfall and four weeks as normal. The sunshine in five weeks has been abundant, in six weeks scanty, and in two weeks normal.

THE S.E. TRADES AND RAINFALL.—Some years ago Dr. W. N. Shaw called attention to a remarkable apparent relation between the strength of the S.E. Trades, as recorded at St. Helena, and the rainfall in England, and the suggestion then made has stimulated research in various directions. In a paper just published in the *Quarterly Journal of the Royal Meteorological Society*, Mr. J. I. Craig, of the Egyptian Weather Service, expresses the opinion that a distinct connexion has been traced between the winds at St. Helena and the Rainfall in Northern Africa. If this opinion is confirmed on further investigation, it will be of great practical importance; for the Nile flood depends on the rainfall in Abyssinia and the adjacent regions, and any reliable prognostication of the probable Nile flood should be of immense advantage to Egypt.

SNOW IN JOHANNESBURG.—Mr. H. E. Wood, in a paper in the *South African Journal of Science* for September, says: "On the morning of August 17th, 1910, the town of Johannesburg, for the first time in its history, was covered with snow to a depth of several inches. To many of its inhabitants, particularly the younger generation, the sight of snow was quite new. The result was that the unusual event was celebrated by a general holiday in the town."

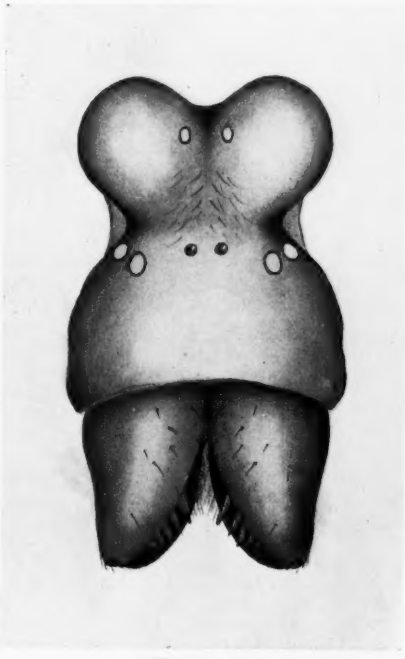

FIGURE 1.

Thyreosthenius biovatus, male. Front view of head showing cephalic prominences and extraordinary arrangement of eyes.

MICROSCOPY.

By A. W. SHEPPARD, F.R.M.S.,

with the assistance of the following microscopists:—

ARTHUR C. BANFIELD. ARTHUR EARLAND, F.R.M.S.
JAMES BURTON. RICHARD T. LEWIS, F.R.M.S.
THE REV. E. W. BOWELL, M.A. CHAS. F. ROUSSELET, F.R.M.S.
CHARLES H. CAFFYN. D. J. SCOURFIELD, F.Z.S., F.R.M.S.
C. D. SOAR, F.R.M.S.

A MYRMECOPHILOUS SPIDER (*Thyreosthenius biovatus*).—A good deal of interest always attaches to the heterogeneous assemblage of creatures which for some reason or other are permitted to spend their existence, or part of their existence, in company with various species of ants, and a few remarks, therefore, concerning a small spider somewhat notorious in this respect, may not be altogether out of place here.

Many species of spider have been, at one time or another, recorded as having been found in ants' nests, but, upon careful investigation of their claims, only three of our British spiders can be considered to be really myrmecophilous, namely, *Tetrilus arietinus*, *Evansia merens*, and *Thyreosthenius biovatus*. It is easy to comprehend how mistakes are liable to arise in this matter. Many small spiders are found under stones, or amongst the vegetation growing against the sides of pieces of rock; and when the protecting body is turned over it is the simplest thing in the world for the spider to drop into a nest of ants should there be one underneath. Spiders, too, are often captured apparently wandering upon the surface of the nest of the wood-ant (*Formica rufa*); but it will be noticed that it is usually in the case where a large nest has been disturbed, and the surviving ants have constructed a comparatively small nest upon the summit of the damaged one, leaving a bulk of uninhabited material upon which intruding creatures may venture with some amount of safety. In several such instances in a wood in the North of London I have found large numbers of a small spider (*Tiso vagans*) actually living amongst the débris of the nest, but in no case were the spiders found in the central portion where the much-persecuted ants still held their own. It would, however, have been extremely easy to have fallen into error, especially as the nests were of a lighter and more friable character than is usually the case, and it was consequently by no means easy to decide where the inhabited portion ended and the débris commenced.

Thyreosthenius biovatus is, in the male sex, a most grotesque and striking creature; but, on account of its small size, its peculiarities are hardly visible even to highly trained eyes, unless a lens be employed. The female, on the other hand, is a most ordinary looking creature, and indistinguishable, except by careful microscopical examination, from quite a number of allied spiders. Both sexes, even when fully developed, are of a somewhat pale colour, and convey the impression of immaturity.

The only species of ant with which they associate appears to be the wood-ant (*Formica rufa*). So far as I am aware the reason for their presence in the nest is quite a mystery. It seems pretty clear that they do not feed upon the ants, which are in size and fighting capabilities vastly superior to them; and it seems unreasonable to suppose that they destroy the various small creatures which the ants harbour as scavengers or pets. Still, the so-called intelligence of the ant has been enormously exaggerated, and it is not beyond the range of possibility that the spider may be fooling its hosts, and perchance making meals at the expense of their offspring when the nurse's back is turned. When the nest is disturbed the spiders do not feign death, but generally extricate themselves from the débris and are quite easily seen. Isolated specimens are occasionally found wandering at some considerable distance from any nest, but, so far as I have been able to ascertain, not in any district where the wood-ant does not occur. As a rule the males appear to be considerably rarer than the females, but it is quite possible that, as in the case of many other spiders, the life of the adult male is a short one.

In some of the very restricted pine areas of south-east Sussex, during the early part of October, specimens of *Thyreosthenius* are sometimes to be found in the small newly-formed nests which are plentiful where larger nests have been destroyed by the gatherers of "ants' eggs." These nests vary considerably in character, some being very rich in pupae and others being almost devoid of any but adult ants. It is a curious fact that I have never found a single spider in a nest where pupae were very numerous. At the season mentioned, namely, early October, the majority of the captures were males, and almost all of these, as well as the females,

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had only just attained maturity. I am unable to report on the contents of these small nests early in the summer as they were not then in existence. Large nests, however, examined in May, contained, on the testimony of local collectors, a comparatively large number of females, the males being stated to be very rare in the adult state.

Dr. Randell Jackson, who has kindly forwarded me a northern specimen of this species, also reports a great preponderance of females amongst his captures. A detailed description of the spider would be out of place here, but the following characters will suffice to distinguish it from its allies:—Eyes small and very widely separated; front row straight by their bases in female, by their centres in male. Posterior eyes strongly procurved (in the male this row of eyes is distorted by the exaggeration of the caput). Legs fairly long. Femora, each with two rows of bristles on the underside. Tibial spine very small, close to base of joint. Metatarsi I, II and III with sensory bristle; in III it is just beyond the middle; in I it is about two-thirds from the base. Tarsi not much shorter than metatarsi; somewhat fusiform, especially in the male.

The species most likely to be confounded with *Thyreosthenius biovatus* is *Peponocranium ludicrum*, which agrees with it pretty closely in size and general appearance, and which possesses, in the male sex, a somewhat similar cephalic protuberance. It occurs also in pine and heath districts, and is not uncommon in many parts of the South of England. The specimen purporting to be a male *Thyreosthenius*, presented to the British Museum by Mr. Donisthorpe, is, in reality, nothing but *Peponocranium ludicrum*.

F. P. SMITH.

QUEKETT MICROSCOPICAL CLUB.—November 22nd, 1910, Mr. E. J. Spitta, L.R.C.P., Vice-President, in the chair. A paper on "A Water Mite new to Britain (*Neumania triangularis*, Piersig)" contributed by Mr. Geo. Plant Deeley. This genus was formerly called Cochleophorus, and several species are described under this generic name by Mr. C. D. Soar, in *Science Gossip*, June, 1900. The locality from which Mr. Deeley obtained his specimens was Parkhill, Stourbridge. Both sexes were taken.—Mr. T. B. Rosseter, F.R.M.S., on "A new species of Avian Tape-worm (*Hymenolepis upsilon*)." He found it in January, 1910, parasitic, amongst the faeces of the intestine of a Wild Duck (*Anas boschas*). The author gave an account of its affinities, references being made to the work of Schilling (1823), Creplin (1829), Dajardin, Pagenstecher (1858), Pfaff and Olrix, and Krabbe (1867). A full description of the specific characters of the new species was given, and from the similarity of the uterine sac to the Greek small *υ*, the name upsilon was bestowed on it.—Mr. James Burton on *Botrydium granulatum*. "The plant consists of small green balloon-shaped vesicles about three millimetres in diameter. Below the surface of the mud an extensive root system is produced; the roots branch dichotomously and extend to a considerable distance, often reaching a length of six millimetres. As the plants very often grow thickly side by side, the roots are entangled and interlaced in the mud, and, the whole plant being of somewhat tender consistency, it is difficult to extricate perfect specimens. The upper part, when young, is filled with protoplasm and is bright green in colour. Later the centre becomes more watery and the green is seen to be due to finely granular chlorophyll. The colourless roots are filled with watery protoplasm. As to its classification, Professor West classes it with the group *Heterokontae*. All the older books put it among the *Siphoneae*, and it seems, for several reasons, to be very appropriately placed there. One of the interesting facts connected with *Botrydium* is that, although it is of such considerable size, it is unicellular. In thinking of a unicellular organism—plant or animal—we usually consider it as something very small, or even microscopic, in size; but that is not necessarily the case. The *Siphoneae* are a class grouped together because of the noticeable characteristic that they are all unicellular, notwithstanding which, most of them are of considerable size. The best-known member of the group is the exceedingly common *Vaucheria*. There are many species—some terrestrial, and found on the damp earth of flower-pots and on damp garden-paths; others occur in ponds. In these plants, though they reach a length of several inches, and sometimes nearly a foot, no dividing-walls appear in the filaments under normal vegetative conditions. Most of the family are marine and one genus—*Caulerpa*, occurring in the Mediterranean—reaches a length of several metres, and yet is only one cell.

Mr. Herbert F. Angus, of the firm of H. F. Angus & Co., exhibited and described a number of Microscopes by R. Winkel, of Göttingen, who, he said, enjoyed a high reputation in Germany, although almost unknown in England. He pointed out that the model was of the more or less stereotyped Continental pattern, inclining more to the Zeiss model than any other, but differing in several important details, as was only to be expected in the productions of an old-established firm who had given proof of their originality in the past by first employing fluorite in the construction of objectives, and producing an objective with a hyper-hemispherical front lens.

A ROCK GRINDING MACHINE FOR AMATEURS.—Nearly every amateur microscopist who has taken up the study of petrology has at some time felt desirous of preparing thin sections of rocks for himself instead of purchasing them, but has in a good many cases been deterred by the labour involved in grinding them by hand. This method will answer the purpose if only a few slides are required, but when he takes up the matter seriously and wishes to make large numbers of sections the student finds the necessity of having some mechanical means to minimize the work.

The professional lapidaries' lathes, and also the special rock slicing and grinding machines placed on the market for petrologists, are generally speaking far more expensive than the average amateur can afford, and I therefore wish to describe a home-made apparatus which has been found to give good results, and to be fully capable of doing the work for which it was devised.

Figure 2 gives a general view of the entire machine, and Figure 1 shows the top of the table and the way in which the various parts are disposed.

The cost of this machine complete is, roughly speaking, 17s., and it can be made by anybody capable of using ordinary tools.

The apparatus is erected on an ordinary sewing machine table, which can be procured very easily. For the slitting disc, which is shown on the right hand side of Figures 1 and 2, it is necessary to get an ordinary polisher's lathe head. This is threaded at one end to take a circular saw or emery wheel, and upon this thread is run the cutting disc, which is then bolted on in the same way as a circular saw. I use a plain disc of soft iron six inches in diameter, and about one-fiftieth of an inch thick.

The rock-holder is the most elaborate part of the arrangement, but is quite simple to manufacture. Figure 3 is a near view of this on a larger scale. It consists of a spindle six inches long, an upright to carry the same, and the jaws to hold the rock. Four and a half inches of this spindle are three-eighths of an inch in diameter threaded sixteen to the inch, and the remaining one and a half inches are a quarter of an inch in diameter, and threaded to take the cones of an ordinary bicycle pedal. This spindle was made for me by a working cycle maker. It is carried on an upright cut out of oak three-quarters of an inch thick. This has a threaded plate screwed on each side of it, through which the spindle moves. There is a lock nut on the spindle behind the upright to hold it rigid. To the end of the spindle are attached the jaws for holding the rock. This portion consists of a piece of oak three quarters of an inch thick, three and a half inches long by three inches wide. To the bottom of this is screwed a piece of oak a quarter of an inch thick, and on top of this is a moveable piece which is clamped on the rock by the butterfly nuts of the two three-inch bolts.

The upright portion of the jaws is fastened to the spindle by the ball bearings of an old bicycle pedal. When the

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machine was first made this bearing was plain, and consisted of a nut and washer behind the upright of the jaws and another nut and washer in front, and these were simply screwed together. This worked fairly satisfactorily, but stuck somewhat when tightened sufficiently to prevent shake, and the bicycle pedal bearing was substituted. It will be seen that the jaws, if properly adjusted, swing on the ball bearings parallel to the cutting disc, and the rock can be moved forward for cutting another slice by turning the spindle toward the operator.

A disc of brass one and three-quarter inches in diameter is fixed to the left hand end of the spindle as a handle for turning it by, and this is graduated into four equal divisions for reference.

In order to get the rock-holder to swing strictly parallel with the cutting disc, some adjustment is necessary, and this is arranged by bolting the upright carrying the spindle to the table. The bolt in front is a fixture, but the one in the rear goes through an elongated hole in the table, so that the upright can be swung on the front bolt as a pivot until the jaws are parallel, and then the back bolt is clamped up tight.

The rock is kept against the cutting edge of the disc by the chain and weight attached to the rock holder at the bottom corner. This can be seen in the illustrations.

The grinding lap is a cast iron disc eight inches in diameter, half an inch thick in the centre, with the top surface bevelled off to three-eighths of an inch at the edge. A lead or copper lap can be used instead of the cast iron one, but I find the latter is quite satisfactory and is cheaper to purchase.

The spindle for this lap is made from an ordinary half-inch bolt, six inches long, procurable from any ironmonger. This requires cutting down until it is about four inches long, then it has to be centred, and made with a cone at the bottom end and a cup at the top. A pulley wheel is fixed at the bottom of this spindle to take the driving band. Two nuts are wanted for the top of this spindle, one of which is screwed up tight for the lap to rest on, and the other to fasten it by. The bearing for this spindle under the table is a screw with a cup in the end, which is fixed with a couple of nuts through a threaded plate. This plate is attached to a piece of wood affixed to the underside of the table. The top bearing is a screw with a cone point which passes through a threaded plate into the top of the spindle. This screw is adjustable for taking up wear and is securely fixed by the lock nut. All this can be procured from a small cycle maker at a very reasonable price.

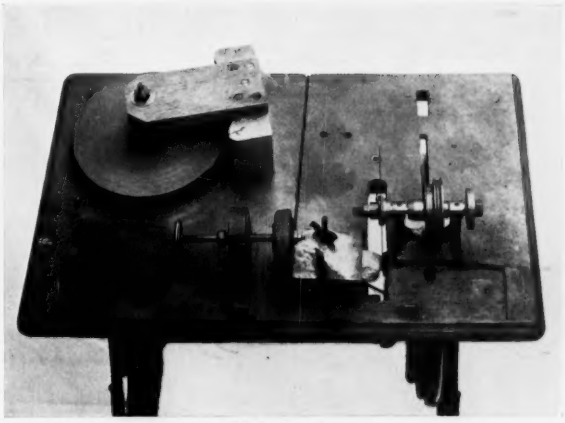


FIGURE 1.

FIGURE 2.

The carrier for this top bearing consists of a block of wood seven inches long by three inches high by two and a half inches wide, and this is securely screwed to the table from underneath.

Upon this is a piece of oak three quarters of an inch thick, seven inches long by three inches wide, to which is screwed the threaded plate for the coned screw of the bearing.

This method of fixing has been found quite satisfactory. It is perfectly rigid, and there is no shaking or jumping of the lap, even when driven at the highest speed obtainable.

It will be seen that the drive for the slitting disc is taken direct from the driving wheel, but that for the grinding lap goes over two jockey pulleys so as to get the horizontal movement. These jockey pulleys are ordinary iron pulley wheels with screw ends, such as are used for carrying blind cords.

It is necessary to have two leather or gut bands, and these have to be changed when altering from the slitting disc to the grinding lap.

A piece of tin, shaped like a bicycle mud guard, is fixed to the table behind the slitting disc to stop the splashes. A tin tray is also placed below the table under the disc to catch the waste carborundum and water, which can be used over again. Round the grinding lap I use a cake tin, which has had a large hole made in the bottom for the spindle and pulley wheel to pass through. A ledge of tin is soldered all round this hole to keep the water, and so forth, from the bottom bearing. These mud guards are not shown in the photographs.

This apparatus has been in use now for about three years, and has ground between four hundred and four hundred and fifty slides during that time. It has given the greatest satisfaction to me, and although, no doubt, it is not so good as a professionally made machine (with which, of course, it is not intended to compete), it will be found a very useful article for the amateur petrologist, who usually has far more enthusiasm than he has money.

C. H. CAFFYN.

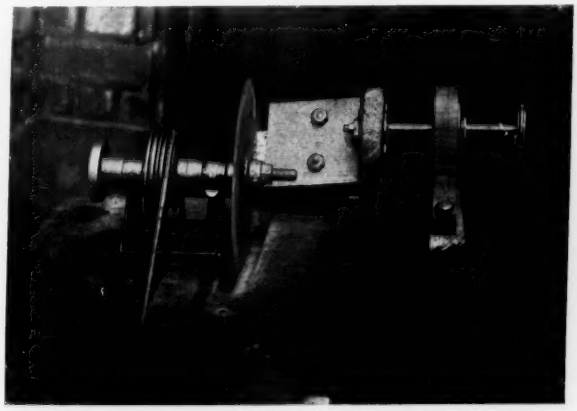


FIGURE 3.

ROYAL MICROSCOPICAL SOCIETY.—November 16th, 1910, Professor J. Arthur Thomson, M.A., F.R.S.E., President, in the chair. In connection with the gift of two slides of *Pleurosigma*, the Hon. T. Kirkman, the donor, asks whether fresh-water Pleurosigmae with oblique striae are rare.

Mr. M. J. Allan, of Geelong, sent a slide of spicules of a species of *Gorgonia* unknown to him concerning the identification of which he sought information. He also sent some slides mounted in fluid by a method of his own. He likewise mentioned in a letter that four years ago he had devised a variable eye-piece giving three magnifying powers, which he offered to send for inspection. Mr. Merlin sent, for exhibition, three photomicrographs of Grayson's Rulings, which Mr. Grayson had sent him. They showed the 100,000, 110,000, and 120,000 bands of Grayson's twelve-band plate. Previously no photograph had ever been exhibited of anything over 112,595 lines to the inch. Dr. Butcher exhibited a number of photomicrographs of diatoms, *Coscinodiscus asteromphalus* and *C. omphalanthus*, *Navicula Smithii*, *Pinnularia cardinalis* and *Triceratium favus*. The photographs were taken in series and represented successive focal planes of each object. The conditions under which they were taken were given.

Mr. A. E. Hilton exhibited a number of mounted specimens of British Mycetozoa. In reference to his exhibit he said that at the present time the disposition was to classify the Mycetozoa among the Rhizopoda, but their metamorphoses marked them off as an entirely distinct group. Their life-history presented three principal phases—aquatic, amoeboid, and aërial. The question of the sexuality of the Mycetozoa was a very obscure one. In all three phases there were nuclear divisions.

The President made a communication on Japanese Pennatulids, and exhibited some typical specimens of great beauty. He said that he had been entrusted by Professor Ijima, of Tokyo University, with a collection of Pennatulids, on the study of which he was at present engaged. His report was not yet ready for publication, but he had thought that it would be of interest to the Fellows of the Society to see a representative sample of these beautiful Sea-Pens. They would understand, when they looked at the dimensions of the specimens, why it was necessary on a long railway journey to be content with a sample of the collection.

The Pennatulacea, or Stelechotokea, include some of the most beautiful of fixed marine animals—long graceful colonies, often plume-like, as their name suggests, with rich colouring, and with strong luminescence. They live fixed on the floor of the sea, and many of them show a familiar adaptation to life on the bottom—long stalks raising the polyp-bearing portion off the substratum. In deep-water forms, such as the beautiful Umbellulas, the proportion of sterile stalk to polyp-bearing rachis reaches an extreme.

The Pennatulids were related to Alcyonarians, such as Dead Men's Fingers, Precious Coral, Organ-Pipe Coral, the Gorgonids and the Gorgonellids, like *Hicksonella*, which Mr. Simpson had established as a new genus at the last meeting of the Society. They differed markedly, however, in several respects. In a very remarkable way the primary polyp, which developed from the fertilized egg, was sacrificed to forming the main axis on which the secondary polyps were borne, which in turn might give off (always through the intermediation of stolons or solenia) tertiary polyps and so on. A central rod, which was present in the majority as the skeletal support of the colony, ran up the middle of the gastric cavity of the primary polyp, and some authorities regarded it, therefore, as endodermic in origin, whereas the skeletal support of all related forms is ectodermic. Thirdly, the Pennatulids almost always showed a pronounced dimorphism—along with the ordinary polyps or autozoöids there were dwarf polyps without tentacles, the siphonozoöids, whose office it was to keep currents of water going in the canals of the colony. It should also be noted that there was in Pennatulacea a marked tendency to bilateral arrangement of the polyps, similar to the arrangement of barbs on a feather.

The President also read two papers by Dr. Jas. F. Gemmill, (1) "Aërator for small Aquaria," (2) "Adaptation of Ordinary Paraffin Baths for Vacuum Embedding." Mr. J. E. Barnard read papers "On the Use of a Metallic Electric Arc in Photomicrography," and on "A Simple Method of obtaining Instantaneous Photomicrographs."

THE EFFECT OF GRAVITY ON *EUGLENA VIRIDIS* EHRB.—At the Meeting of the Royal Society held on November 17th, Harold Wager, F.R.S., gave the results of some experiments on the movements of micro-organisms. *Euglena viridis*, when placed in the dark in shallow vessels or narrow tubes, becomes aggregated into network-like patterns or more or less well-defined circular groups. If a narrow tube filled with water containing sufficient Euglenae to give it a pronounced green colour be placed horizontally in the dark or in a weak light, the aggregation takes the form of a series of nearly equally spaced groups like green bands crossing the tube from one side to the other. Each group shows clearly two distinct regions, a central denser one consisting of cells moving downwards, and a lighter peripheral area consisting of cells moving upwards. There is, in fact, a constant cyclic movement, which is kept up so long as the aggregation persists. Examination with a pocket lens shows that, as the organisms reach the bottom of the stream, they gradually separate from one another, and begin to move upwards. As they reach the upper surface, they are seen to be drawn towards the central denser region of the group, and again enter the downward stream. This aggregation, with its regular cyclic movements, may persist for several days, provided the Euglenae are kept in the dark or under red glass.

The downward movement appears to be a purely mechanical one, dependent upon the specific gravity of the organism, and is not due to a stimulus which evokes a physiological response as in geotropism or geotaxis. The upward movement is, on the other hand, due partly to the activity of the organisms themselves, partly, no doubt, to the currents set up in the liquid by the friction of the downward moving stream. The upward movement of *Euglena* appears to be controlled, so far as the orientation of its elongate body is concerned, by the action of gravity.

The network-like aggregations and groupings resemble very closely the cohesion figures which are formed, under certain conditions, when fine sediment is allowed to settle slowly in a liquid, and the conclusion has been arrived at that the aggregations are probably of the nature of cohesion figures, due to the action of gravity upon organisms massed together, combined with the vortices set up by the streaming movements.

The movements of certain micro-organisms are therefore controlled in a purely mechanical fashion, and the advantage to those species which, like *Euglena*, are often found in a confined space in large numbers, must be very great, as by its means a constant circulation is maintained, and they are prevented from accumulating in such dense masses as would be detrimental to their existence by interfering with their assimilatory or respiratory functions.

PHOTOGRAPHY.

By C. E. KENNETH MEES, D.Sc.

INFRA-RED PHOTOGRAPHS.—A recent holiday trip to Portugal gave me an opportunity of trying some landscape photographs taken by infra-red light after the manner suggested by Professor R. W. Wood.

A filter was prepared which passed only light above 7,250 A.U., and through which, therefore, only a very faint amount of deep red light could be seen by screening the eye thoroughly.

The plates which were used had a sensitiveness extending with long exposures to 7,600, so that the region used with the plates and screen was situated between the *a* and *A* lines, from 7,250 to 7,600.

The multiplying factor of the screen on these plates was found to be about three thousand, so that, as a normal exposure at F. 8 in sunlight would have been about one-tenth of a second, the infra-red photographs required an exposure of five minutes.

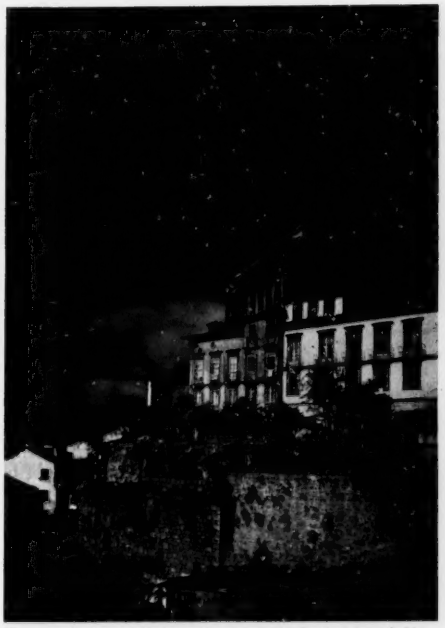
Unfortunately the peculiarities of the Portuguese small boy, and the very limited time at my disposal, tended to cut

exposures too short; but some of the results obtained proved of considerable interest.

Two typical examples are reproduced herewith. The first is taken from the great bridge over the Douro, in Oporto, and shows well one of the most striking features of all these photographs—the intense blackness of the blue sky. (See Figure 1.) Even the faintest trace of cloud stands out at once against the blue sky when observed by infra-red light, so that it would seem possible that the method might be of considerable use to the meteorologist. The shadows in infra-red light are extraordinarily deep, showing no detail at all if the sky be clear, because the blue skylight contains so little light of long wave length.

The second photograph, which is taken in the Avenue Liberdad, in Lisbon, shows the extraordinary appearance of foliage under this infra-red light. The chlorophyll has but the slightest absorption for it, so that the foliage on the trees appears white, while in the left-hand bottom corner we have the curious phenomenon of a white palm! (See Figure 2.)

Incidentally, it is interesting to remember how few coloured substances do absorb infra-red light between seven thousand and eight thousand A.U. The only two dyes which I know to absorb completely in this region are Naphthol Green and the new Hoechst dye, Filter Blue-Green. The new Badische Anthraquinone dyes absorb to seven thousand five hundred but transmit light of greater wave length; nearly all other blue and green dyes transmit red light of wave length seven thousand or under.



From an Infra-red photograph by C. E. Kenneth Mees.

FIGURE 1.

A view from the great Bridge over the Douro in Oporto.

AN "EXPLOSION" THEORY OF THE LATENT IMAGE.—In a letter to *The British Journal of Photography*, Mr. F. F. Renwick makes a suggestion for a new theory of the latent image, though various facts pointing in the same direction have been published at intervals. This theory is based on the observation of Dr. W. Scheffer, that a silver bromide grain on exposure, violently throws off a part of its substance, rupturing the surrounding gelatine in its passage.

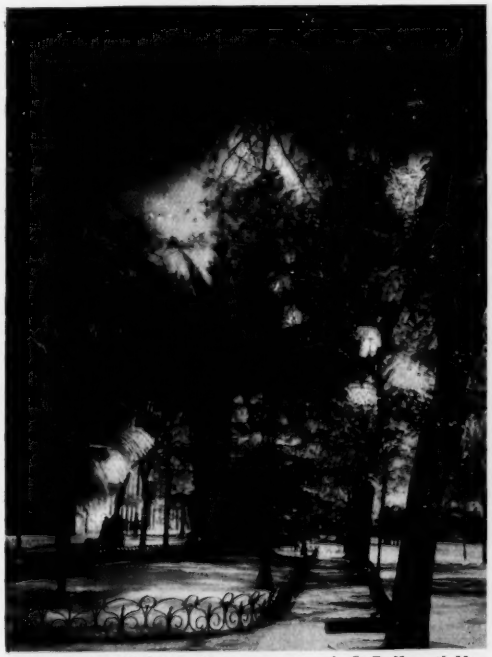
Mr. Renwick suggests that, in an emulsion, the silver bromide grains are wrapped round by a tangled meshwork of gelatine, and can only be attacked either through the extremely minute channels left, or by diffusion through the substance of the gelatine skeleton. Now, if Dr. Scheffer's observations be accepted, then in the neighbourhood of an exposed grain the densely tangled network is broken through, and channels of relatively large size, giving far readier access for the developer to the silver bromide grains, are formed.

In some respects this theory seems to be more satisfactory than any hitherto held. It has generally been assumed that the commencement of development depended upon the provision of a nucleus, upon which the silver produced by the interaction of silver bromide with the reducer could precipitate. This remains probable, but a difficulty was that, in this case, once fogging from an unexposed developer had commenced, it should have proceeded at the normal rate. This does not seem to be the case; if measurements be taken of the increase of fog with time of development for an unexposed plate, it is found that the function obtained is similar to that given by an exposed plate, but with a much lower velocity constant.

This is accounted for at once if the exposed grains have literally become "exposed" to the attack of the developer, by blasting passages through their surrounding network.

This explosion theory is also valuable in that it enables one to give a meaning to the "ripening" of an emulsion. A "ripened" grain would be one which was in the most explosive state; that is, in which the crystallisation occurring during cooking had reached the limit of stable equilibrium, so that any further access of energy would result in its disintegration.

The theory is certainly fascinating in its possibilities, though it will have to face much criticism, especially from a consideration of the destruction of the latent image by oxidisers, and of the desensitising action of some metallic salts, when added to the emulsion.



From an Infra-red photograph by C. E. Kenneth Mees.

FIGURE 2.

The Avenue Liberdad in Lisbon.

PHYSICS.

By W. D. EGGAR, M.A.

THE CAVENDISH LABORATORY.—On Saturday, November 12th, a large and distinguished company assembled at "the Cavendish" to do honour to Sir J. J. Thomson, whose twenty-five years of office as Professor of Experimental Physics have been signalized by the publication of the "History of the Cavendish Laboratory." The Vice-Chancellor presided, and Dr. Glazebrook, of the National Physical Laboratory, presented a specially bound copy to his old friend and former chief. Few institutions possess a more distinguished record than this Cambridge Laboratory, young as it is. James Clerk Maxwell was appointed as first professor to the newly constituted chair in 1871. The Laboratory, built under his directions, was opened in 1874. When Maxwell died, in 1879, Lord Rayleigh succeeded to the post, which he held until 1885. J. J. Thomson, then a very

young man, was the third professor, and under him the Cavendish has, to quote Lord Rayleigh, assumed the first place among physical laboratories. Not only the professor's own researches, but the spirit with which he has animated the band of students who have thronged to him at Cambridge, have spread the fame of the laboratory throughout the civilized world.

SCIENCE AND ENGINEERING.—Sir J. J. Thomson, in a presidential address to the Junior Institution of Engineers, pointed out that the distinction between Physics and Engineering is one of aim, and not of method. It is the Engineer's business to turn to practical account the advances made by the Physicist. But the latter must by no means concern himself with utility. No man can foresee the significance of a new discovery; and it would be disastrous to the progress of Engineering if men of science were to confine their researches to matters of obvious utility. At the same time England is still behind Germany in the way in which new discoveries are seized upon and applied industrially. For example Professor Dewar invented a flask for holding liquid air. A form of this is now sold in large quantities as the "Thermos" flask, and used for keeping tea and other forms of refreshment hot (or cold). But none of these flasks are made in England. A well-known example of the German faith in pure scientific research exists in the Jena glass industry, which owes its foundation to the patient and thorough investigations of Abbé and Schott.

ZOÖLOGY.

By PROFESSOR J. ARTHUR THOMSON, M.A.

HABITS OF THE WOMBAT.—J. A. Kershaw got two living wombats (*Phascolomys ursinus*) from Flinders Island in Bass Strait, and kept them alive in the National Museum, Melbourne. They took fresh grass and thistles readily, and allowed themselves to be handled. One of them had a young one in the pouch, which emerged in fifteen days. But it did not survive long. "In habits these animals remind one of the Rodents, their manner of feeding and quick side-to-side movement of the jaws being very similar. They are very quick in their movements when excited or alarmed, and run with greater speed than one would expect from such an apparently awkward animal. When touched, especially near the hind quarters, they have a peculiar habit of kicking violently backward with both hind feet. This, it was noticed, occurred even when approached by its companion. If annoyed, they do not hesitate to use both teeth and claws." A peculiarity not before noticed is their habit of closing their claws on the rough under surface of the paw so as to grasp pieces of grass and the like. They spent most of the day sleeping, partly buried in their bedding.

SPERMACETI ORGAN.—E. Danois has studied this curious organ in the whale, called *Kogia breviceps*, and finds that it closely resembles that of the cachalot. He finds corroboration of the view of Pouchet and Beauregard that the spermaceti organ is a dependence of the right nostril, and equivalent to a mucous gland in other toothed Cetaceans, such as the dolphin. If so, we have another illustration of a frequent evolutionary method—making an apparently new organ by a transformation of a very old one.

WASHING OYSTERS.—Fabre-Domergue has made an interesting series of observations at Concarneau, which should serve to justify scientific methods in the eyes of the world, for they concern the oyster. He has shown that oysters may be kept for eight days (or even for a fortnight) in filtered water, frequently changed, without losing any of their virtues, but gaining rather. The micro-organisms which are apt to linger in the mantle cavity, with deleterious results to the oyster-eater, can be thoroughly washed away by the filtered water, and the oysters do not lose in weight, nor in their power of "vital resistance," nor in "embonpoint." Their market value is unaffected, and we can swallow them with a lighter heart.

COLOUR SENSE OF HIVE-BEES.—John H. Lovell has made an experimental contribution to a much discussed question:—"Do Hive-bees distinguish colour as such?" The results of his experiments strongly support the conclusion that bees distinguish colours. They are more strongly influenced by a coloured slide than by one without colour, and when they get accustomed to visit a certain colour they tend to return to it habitually. They stick to their colours. But "this habit does not become obsessional."

ARE THERE BLACK CORALS IN THE NORTH SEA?—Messrs. Freeland, Fish Merchants, Aberdeen, recently presented to the Aberdeen University Museum a beautiful Antipatharian, or Black Coral, with two thick irregular branches over a yard in length, and with a basal diameter of nearly an inch! They got this from a trawler, which reported finding it some fifty miles off Aberdeen. Such records must be taken *cum grano salis*, for the sense of accuracy varies greatly in its degree of development, and mistakes may arise without any intention to play a trick on the innocent naturalist. Many of the trawlers make long voyages nowadays, a specimen may be passed from hand to hand, and invention may be called upon to supply what memory has lost. On the other hand, the specimen is rather an awkward one to carry about, and a secure record in 1908 of a large Antipatharian (*Parantipathes larix*) from the north-east of the Faeroes makes one more inclined to admit that the locality reported may be accurate. Unfortunately, the beautiful ebony black axis is polished from end to end, and there is not a trace of a spine, far less of a polyp! Secure specific identification is almost impossible.

COLOUR IN DEEP WATER.—Frederick Chapman has called attention to the occurrence of deeply coloured tests of the Foraminifer (*Polytrema miniaceum*), at a depth of five hundred and seven fathoms. The species inhabits relatively shallow water; the specimens from five hundred and seven fathoms showed the characteristic rose-pink colour. It may be recalled that one of the results of Sir John Murray's 1910 Expedition is to extend the light limit. Distinct traces of light were detected at five hundred fathoms.

REVIEWS.

ASTRONOMY.

The Romance of Modern Astronomy.—By HECTOR MACPHERSON, JUNR. 334 pages. 39 illustrations and diagrams. $7\frac{3}{4}$-in. × $5\frac{1}{4}$-in.

(Seeley & Co. Price 5/-.)

This volume is one of the series published by this firm under the title of "The Library of Romance." We do not welcome works of romance in astronomy, preferring books dealing more with hard facts and less with romance and supposition, leaving such as these to be supplied by the readers. However, in the present instance, we do not find fault with the book, only with the title, for the author has written an entertaining book, and we like his way in which he records, in a pleasant and accurate manner, most of the great and well-known astronomical discoveries of the past four hundred years, as well as some of the Grecian discoveries. We would scarcely consider it as a text-book or book of reference for an observatory library, though a copy should be in all observatories; but few astronomers, and none of the

general folks who love to read about astronomy, should fail to read, better still, acquire, this book. It is a book well suited for municipal and lending libraries, which mainly exist for exciting and stimulating a reader's interest in a subject.

The printing is clear and on light, thick non-surfaced paper; there are thirty-three chapters, and more than a dozen excellent plates, reproduced from photographs by Max Wolf, Puiseux, Janssen, and Lowell; there is also a good index.

B.

Ball's Popular Guide to the Heavens.—Third and Revised Edition. 96+xii. pages. 84 maps.

(George Philip & Son. Price 15/- net.)

We are glad to see this indispensable volume kept to the fore, by a third edition five years after the second edition.

In 1892 Sir Robert Ball, greatly aided by the present Radcliffe Observer (Dr. A. A. Rambaut), who formed two-thirds of the seventy-two plates, besides a large portion of the seventy-two pages of letter-press and index, wrote the first edition of this guide, then called *An Atlas of Astronomy*; that work consisted of seven chapters of text, which preceded the seventy-two plates. In the second and third editions a small number of these maps, chiefly of planets and comets, have been omitted or superseded by later work, and we find such names as Barnard, Ritchey, Hale, Campbell, Keeler, and so on, supplementing or replacing work by Common, Green, Roberts, Henry, Proctor, and others; astronomical progress demanded these changes. So that the changes result in a volume of eighty-four maps and ninety-six pages of letter-press in nine chapters.

The maps which were not included in the first edition, are chiefly those of the nebulae, comets, and sun. An important change, and an improvement upon the first edition, is in printing the maps of the tracks of the planets and the star maps upon a blue ground.

The maps are for the most part so excellently reproduced and cover the ground of astronomy so well that it is difficult to know where to find fault. But there are a few faults to which, in view of a fourth edition, we draw attention, as they have already been reprinted from the second edition. On page 13, Saturn's rings "are now (1903)." This is incorrect; the sentence required re-writing; page 17, a misprint for shadow-patch, also wrong in second edition; page 60 and other pages, deceased astronomers are sometimes referred to as "late," sometimes not; page 62, we think the number of stars in the Astrographic Survey Catalogues will be nearer four millions of stars than two millions, and the year (1903) is wrong. We also notice on page 54 that the maximum magnitude of Nova Geminorum is given as seven; it should be five, or even brighter, and an important omission occurs in the list of new stars, Janson's discovery in Cygnus in 1600; again on page 6, referring to Halley's comet, the author says "and its next return in 1910 will be awaited with very great interest." This was correct for the second edition of 1905; it is rather unfortunate that this should not have been corrected in the present edition, knowing that Halley's comet had come and gone five months ago, and that a frontispiece of this comet is given. Considering the many beautiful photographs of the comet which have been taken by Curtis (Lick Observatory), at the Cordoba and other favoured observatories, it is also to be regretted that this comet's beauty should be so depreciated and so unfavourably compared with the January comet by the drawing given. In future editions we would like to see reproduced the binary star information as in the first edition, but in a popular guide possibly that would be out of place; also a photograph of Halley's comet and some of Hale's beautiful solar work. The distribution of the text among the plates is a step in the right direction, and more appropriate to such works. May this volume reach many more editions and maintain its high character as a book which is needed in the observatory and in the astronomer's library.

F. A. B.

BOTANY.

A Text Book of Botany.—By J. M. LOWSON, M.A., B.Sc. Fifth Edition, Revised and Enlarged. 607+vii. pages. Illustrated. 7-in.×5-in.

(W. B. Clive. Price 6/6.)

Although written specially to meet the requirements of students reading for certain examinations, this work has decided merits, which have led to its being used extensively by botanical students. As a handy and concise compilation, published at a moderate price, it can be recommended to all who wish for a single book to serve for reference, as well as an introduction to more detailed works dealing with the different branches of the subject.

The present edition differs from its predecessors mainly in containing a large amount of matter which has been taken from the works of Professor Cavers, published by the same firm.

Plant Life in Alpine Switzerland.—By E. A. NEWELL ARBER, M.A., F.L.S., F.G.S. 355 pages. 48 plates. 30 figures. 8½-in.×6-in.

(John Murray. Price 7/6 net.)

The wild flowers of the Alps, so numerous and so brightly coloured, cannot fail to attract the summer visitor. He gathers them, and finding them very different from the flowers of his own country, seeks to know their names. To aid him in this, there are a thousand books with gaudily coloured pictures, but should he desire to go a step further and find out something of the life histories of these flowers, and why they differ so markedly from the flowers with which he is familiar, there are very few popular books to which he can turn for guidance.

The volume now before us is in no way a flora, and very little is said as to the identification of the plants described. The introductory chapter deals with points in the structure of two of the best known of Alpine plants, the Alpenrose and the Edelweiss. Typical flowers of the meadows, pastures, rocks, marshes and forests are next discussed, and their special adaptations to the regions in which they are found, and to one another, are explained. The last chapter is given up to a discussion on the affinities and origin of the Alpine flora. A glossary of botanical terms, some notes on the structure of the flower and a bibliography form an appendix.

It has clearly been the object of the author to make the whole book abundantly intelligible, even to those without the slightest botanical knowledge, and in this he has succeeded well. If we must criticise we would almost suggest that in the first chapter the intelligence of the reader is perhaps a little insulted, that even too much water is added to the milk. The book is well illustrated by some excellent diagrams and there are some forty-eight plates, reproduced from photographs, some of which are exceedingly good. There was clearly a definite need for a book of this sort, and we feel that the present volume is admirably adapted to meet it.

GEOLOGY.

Smithsonian Miscellaneous Collections. Vol. 56. No. 5. *A Preliminary Study of Chemical Denudation.*—By F. W. CLARKE, Chief Chemist, U.S. Geological Survey. 19 pages. No. 6. *The Age of the Earth.*—By G. F. BECKER. 28 pages.

(Smithsonian Institution).

Dr. Clarke's paper is occupied with a revision of those geochemical data which are used in the discussion of some of the larger theoretical problems of geology. On the basis of the vast amount of material accumulated by the Hydrographical Department of the U.S. Geological Survey, a new estimate is made of the amount of matter carried down by North American rivers in solution to the sea. For the United States the revised *denudation factor* is seventy-nine metric tons of dissolved matter per square mile of drainage basin per year. Revised estimates are made as far as possible for South American, European, African, and Asian rivers, but the data

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for most of these are still very defective. The composition of the matter thus carried to the ocean is discussed, and a special study is made of the factors used in Professor Joly's method of estimating the age of the ocean by means of the sodium content. Using Dr. Clarke's new figures, G. F. Becker attacks the problem of the age of the earth by Joly's well-known method. The chief new point introduced by Becker is that the increment of sodium to the sea during geological time has not been uniform but asymtotic. He conceives that the Archaean and massive igneous rocks which supply the great mass of the sodium originally occupied a far larger area, and were thus more exposed to denudation than they are now. Consequently the supply of sodium to the sea tends to suffer an asymtotic diminution with the lapse of time. With the new considerations and the revised data, the age of the ocean is now estimated at between 46·0 and 74·4 millions of years. The data for Lord Kelvin's method are similarly revised, and a final estimate of fifty-five to seventy millions of years for the age of the earth (starting from the *consistentior status*) is arrived at, limits not differing greatly from those found by Joly's method.

PHYSICS.

The Principles and Methods of Geometrical Optics.—By JAMES P. C. SOUTHALL. 626 pages. 170 illustrations. 9¼-in.×6-in.

(Macmillan & Co. Price 25/- net.)

The Germans have taken possession of the great province of Applied Optics. The remarkable theoretical work of Petzval, Seidel, and Abbe, together with the systematic investigation, undertaken and carried through by Abbe and Schott, of the "optical properties of all known substances which undergo vitreous fusion and solidify in non-crystalline transparent masses," has led to the establishment of the Jena "Glastechnisches Laboratorium." Not only so, but until quite recently there has been no English treatise on Optics in which any reference to the later German theoretical work could be found. This book is a very successful attempt to supply the deficiency, and it is likely to be found indispensable as a book of reference. After a chapter on the fundamental laws of Optics, the characteristic properties of rays of light are considered, such as the principle of the shortest route, with an account of Sturm's theory of astigmatism. Then follow Reflexion and Refraction at plane surfaces, with a thorough discussion of prisms and prism-systems. After chapters on the reflexion and refraction of paraxial rays at spherical surfaces, comes a full account of Abbe's theory of Optical Imagery, by means of which it is possible to separate theoretical from mechanical impossibilities. The theory of Spherical Aberrations and Colour-phenomena also receive full treatment. A pleasing feature of the book is the fulness with which the history of Optics is treated. The author's affection for his subject is evident, and amid large arrays of determinants and pages of symbols the human element is never lost sight of. Full references to original sources are given, and in particular the author acknowledges his indebtedness to Czapski's great work. The book forms an excellent general introduction to the special theory of optical instruments, and another book dealing fully with the different types of instruments may possibly follow in time.

The Young Electrician.—By HAMMOND HALL. 306 pages. 93 illustrations. 8-in.×4-in.

(Methuen & Co. Price 5/-.)

A thoroughly practical book and one from which, if a boy conscientiously worked (with a little sympathetic assistance) the series of experiments so subtly set forth in the succeeding pages, he would gain an excellent knowledge of the fascinating subject of electricity. Quite unlike the majority of "young" books it is practically written and yet is understandable by the uninitiated; and the final chapter, which deals with Wireless Telegraphy, leaves one like Oliver Twist.

One would suggest, just for boys' books, that the necessaries for the experiments and the construction of the models be tabulated, and the illustrations and diagrams should also be on the opposite page.

PHYSIOLOGY.

The Physiology and Hygiene of Sleep.—By DAVID FRASER HARRIS, M.D., C.M., F.R.S.E. 19 pages. 7-in.×5-in.

(Cornish Bros. Price 3d.)

The Annual Public Lectures on "The Laws of Health" of the Midland Institute, Birmingham, are always interesting and instructive, and this year's lecture, delivered in September, 1910, and here reproduced in the form of a small booklet is certainly one of the best. Dr. Fraser Harris divides the causes that normally lead to and produce sleep into four classes:—The action of fatigue toxins from muscular and mental work; the diminution of the cerebral blood flow; the regular recurrence or, better, absence of sensations from the external world; and lastly the purely psychic causes, the absence of ideas, worries, emotions and the like. He gives us numerous examples of each of these types of sleep and shows how the opposite conditions may give rise to insomnia. Particularly would we agree with him, when he points out the general inconvenience that results from the action of those who go about our streets late at night, singing and shouting, and asks for legislation to stop this practice. Lastly, working along these same lines, some simple but excellent advice is given for the recall of "Sleep, O gentle Sleep, Nature's soft nurse!" by those who have lost her companionship. The booklet contains one of the best descriptions in popular language of the Physiology of Sleep that we have yet seen.

ZOOLOGY.

The Book of the Animal Kingdom.—By W. PERCIVAL WESTELL. 379 pages. 274 illustrations. 7-in.×9-in.

(J. M. Dent & Co. Price 10/6 net.)

Mammals are not classified in this book on any systematic plan, for it is not intended to be a scientific treatise, but, as the writer hopes, will appeal not only to young folks in the homeland and in other places where English is spoken, but also to adults who have a general interest in the subject. The creatures, therefore, are grouped together according to their exceptional size, their means of protection, and their ways of obtaining their prey. The large number of photographic illustrations, by Mr. W. S. Berridge, adds greatly to the attractiveness of the book, and Mr. Westell has gone to some of the best authorities for information that is not within his own experience. We learn that if this book has the reception that it deserves it will be followed by another dealing with birds.

NOTICES.

THE SCIENTIST'S REFERENCE BOOK AND DIARY.—The present form of this useful annual consists of two books joined together but opening different ways. The first of these contains a diary with useful hints with regard to First Aid, and a Daily Wants dictionary, which gives much valuable information that is often required at a moment's notice. The other book consists of tables and various facts, under the headings of the different sciences, which are likely to be wanted by scientific people. A certain number of leaves are also left blank for memoranda.

Messrs. James Woolley, Sons and Company are to be congratulated on the publication. Some of the information with regard to the Scientific Societies is not quite up to date; for the Royal Anthropological Institute, the Zoölogical Society, and the Selborne Society, among others, have changed their addresses recently.

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THE FACE OF THE SKY FOR JANUARY.

By W. SHACKLETON, F.R.A.S., A.R.C.S.

THE SUN.—On the 1st the Sun rises at 8.8 and sets at 3.59; on the 31st he rises at 7.43 and sets at 4.44. The Earth is nearest the Sun on the 3rd, when the Sun attains his maximum apparent diameter of 32′ 35″. Sunspots and faculae may usually be observed on the solar disc; of late the spots have been small. The positions of the Sun's axis, centre of the disc, and heliographic longitude of the centre are given below:—

Date.	Axis inclined from N. point.	Centre of Disc S. of Sun's Equator.	Heliographic Longitude of Centre of Disc.
Jan. 1 ...	2° 16′E	3° 7′	294° 30′
,, 6 ...	0° 11′W	3° 41′	228° 39′
,, 11 ...	2° 36′W	4° 13′	162° 49′
,, 21 ...	7° 18′W	5° 11′	31° 8′
,, 31 ...	11° 40′W	6° 1′	259° 28′
Feb. 5 ...	13° 41′W	6° 20′	193° 38′
,, 10 ..	15° 36′W	6° 38′	127° 48′

THE MOON:—

Date.	Phases.	H. M.
Jan. 8 ...	☽ First Quarter	6 20 a.m.
,, 14 ...	○ Full Moon	10 26 p.m.
,, 22 ...	☾ Last Quarter	6 21 a.m.
,, 30 ...	● New Moon	9 45 a.m.
Feb. 6 ...	☽ First Quarter	3 28 p.m.
Jan. 13 ...	Perigee	0 18 a.m.
,, 24 ...	Apogee	7 42 p m.

OCCULTATIONS.—The following are the principal occultations visible before midnight:—

Date.	Star's Name	Magnitude.	Disappearance.		Reappearance.	
			Mean Time.	Angle from N. point. E.	Mean Time.	Angle from N. point. E.
			p.m.		p.m.	
Jan. 11	υ^1 Tauri ...	4·2	6.11	79°	7.17	237°
,, 11	υ^2 Tauri ...	5·4	6.43	62°	7.52	255°
,, 12	125 Tauri ...	5·1	10.21	107°	11.27	241°
Feb. 7	A^1 Tauri ...	4·5	5.26	8°	6.4	304°

THE PLANETS.

MERCURY:—

Date.	Right Ascension.	Declination.
	h. m.	
Jan. 1 ...	19 53	S 20° 36′
,, 16 ...	18 52	19° 31′
,, 31 ...	19 6	21° 23′
Feb. 15 ..	20 23	S 20° 27′

Mercury is in Inferior Conjunction with the Sun on the 10th January and is thus unobservable for the greater part of the month. Towards the end of the month the planet is a morning star in Sagittarius, and rises on the 31st at 6.28 a.m. or $1^h\ 16^m$ before the Sun. The planet is at greatest westerly elongation of 25° 17′ on February 2nd.

VENUS:—

Date.	Right Ascension.	Declination.
	h. m.	
Jan. 1 ...	19 21	S 23° 11′
,, 16 ...	20 41	19° 45′
,, 31 ...	21 56	14° 12′
Feb. 15 ...	23 7	S 7° 13′

Venus is an evening star in Capricorn, but practically unobservable throughout the month. On the 31st January the planet sets at 6.8 p.m. or $1^h\ 21^m$ after the Sun.

MARS:—

Date.	Right Ascension.	Declination.
	h. m.	
Jan. 1 ...	16 27	S 21° 43′
,, 16 ...	17 12	23° 10′
,, 31 ...	17 59	23° 49′
Feb. 15 ...	18 46	S 23° 35′

Mars rises about 5.30 a.m. throughout the month, and is visible in the S.E. portion of the sky for a short time before sunrise. The diameter of the planet's disc is only 4″; thus, under present conditions, useful telescopic observations are practically impossible.

JUPITER:—

Date.	Right Ascension.	Declination.
	h. m.	
Jan. 1 ...	14 30	S 13° 37′
,, 21 ...	14 41	14° 24′
Feb. 10 ...	14 48	S 14° 52′

Jupiter is a conspicuous object in the morning sky looking S.E.; he rises about 3 a.m. on the 1st January and at 1.20 a.m. on the 31st.

He is in quadrature on the 3rd February. The equatorial diameter of the planet is 34″, whilst the polar diameter is 2″·2 smaller. This polar flattening is readily observed in telescopes powerful enough to see the belts, but the satellites may be seen in small telescopes such as deer-stalkers of about 1¼ inches aperture, or even in a good pair of prismatic binoculars magnifying 8 times. The Moon appears near the planet on the 23rd.

SATURN:—

Date.	Right Ascension.	Declination.
	h. m.	
Jan. 1 ...	1 54	N 9° 0′
,, 21 ...	1 55	9° 12′
Feb. 10 ...	1 59	N 9° 40′

Saturn is due South on the 1st January at 7.13 p.m. and at 5.18 p.m. on the 31st. The planet appears as a bright star

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looking South and about 30° above the horizon; he is very conveniently situated for making telescopic observation, and with his rings he forms one of the finest sights in the sky.

The ring may be seen quite well in telescopes of about 1¾ inches aperture with a magnifying power of 40, if the instrument is sufficiently steady and the object glass good; but larger telescopes are required to see the division in the ring and the belts on the disc. The planet is in quadrature on the 21st, and sets at 11.24 p.m. on the 31st.

As seen in the telescope the ring appears fairly open, since we are looking on the Southern surface at an angle of 16°. The apparent diameters of the outer major and minor axes of the ring are respectively 42″ and 12″, whilst the diameter of the ball is 17″.

The Moon appears near the planet on the 8th.

URANUS:—

Date.	Right Ascension.	Declination.
	h. m. s.	
Jan. 1 ...	19 45 48	S 21° 43′ 46″
Feb. 1 ...	19 53 20	S 21° 24′ 3″

Uranus is in conjunction with the Sun on the 16th January; hence the planet will be unobservable throughout January and February.

NEPTUNE:—

Date.	Right Ascension.	Declination.
	h. m. s.	
Jan. 1 ...	7 28 6	N 21° 15′ 5″
Feb. 1 ...	7 24 28	N 21° 23′ 1″

Neptune is in opposition to the Sun on the 11th, hence about this date he is on the meridian at midnight. The planet is situated in Gemini, about three and a half degrees South-East of the star δ Geminorum, but he is difficult to identify except in large telescopes. He may, however, be detected by his relative motion if successive observations are made at intervals of some days.

METEOR SHOWERS:—

Date.	Radiant.		Name.
	R.A.	Dec.	
	h. m.		
Jan. 2–3 ...	XV. 20	+53°	Quadrantids.
,, 17 ...	XIX. 40	+53°	θ Cygnids.

Minima of Algol occur on the 4th at 11.35 p.m., 7th at 8.24 p.m., 10th at 5.13 p.m., 27th at 10.7 p.m., and the 30th at 6.56 p.m. The period is $2^d\ 20^h\ 49^m$ from which data other minima may be calculated.

TELESCOPIC OBJECTS:—

NEBULAE.—Orion Nebula, situated in the sword of Orion, and surrounding the multiple star θ, is the finest of all nebulae; with a three or four inch telescope it is best observed when low powers are employed.

CRAB NEBULA (M 1), in Taurus, situated about 1½° North-West of ζ Tauri, in R.A. $5^h\ 29^m$, Dec. 21° 58′ N.

CLUSTER.—M 37, situated in Auriga, is one of the finest clusters, and very compact; its position is R.A. $5^h\ 46^m$, Dec. 32° 32′ N.

DOUBLE STARS.—β Orionis (Rigel), mags. 1 and 9, separation 9″. On account of the brightness of the principal star, this double is a fair test for a good object-glass of about three-inch aperture.

δ Orionis, mags. 2 and 7, separation 53″; easy double.

ζ Orionis, triple, mags. 3, 6, and 10, separation 2″·5 and 56″; rather difficult in a three-inch telescope.

λ Orionis, mags. 4 and 6, separation 4″·5; pretty double.

σ Orionis, triple, mags. 4, 8, and 7, separation 12″·5 and 42″.

NOTICES.

YEAR BOOKS.—From Messrs. Adam and Charles Black we have received "Who's Who" for 1911 (price 10/- net). Apart from being indispensable to everyone who is doing any public or commercial work, "Who's Who" contains many facts of considerable interest and not a little humour, as those will find who take the trouble to dip into its concise biographies. From the same publishers comes the "Englishwoman's Year Book" (price 2/6 net), which this year has been divided into two parts, the first dealing with education, professions and social life, and the second with philanthropic work more particularly.

The "Writers' and Artists' Year Book" (price 1/- net), is chiefly of value to literary people and should be of particular use to those who are thinking of getting a living by their pen.

"The Year Book of Scientific and Learned Societies," published by Messrs. Charles Griffin & Company (price 7/6), has for more than a quarter of a century given the more important details with regard to all the scientific and learned societies in this country, and set forth the titles of every paper and lecture read or delivered before them during the year with the name of the author. The issue for 1910 is in every way equal to its predecessors and it is obvious that few scientific workers fail can to get some information or inspiration from a study of its contents.

Among other Year Books which we have received are "Whitaker's Almanack" (price 1/-; in cloth 2/6 net.), and "Whitaker's Peerage" (price 5/- net.), which present certain new features but which are more particularly useful on account of the old ones.

SCIENTIFIC INSTRUMENTS.—We have received from the Cambridge Scientific Instrument Company, two illustrated leaflets. The first deals with a large sliding microtome which is important from a scientific point of view as with it very large sections can be cut from material embedded in celloidin. In point of fact the maximum size is 150^{mm} by 120^{mm}, that is, the largest section will measure $6 \times 4\frac{3}{4}$ inches; and in illustration of the capabilities of the instrument a figure is given of a complete section of a human leg six inches above the ankle, which was prepared and cut by the new microtome at the Anatomy School at Cambridge University.

The other piece of apparatus is an automatic temperature regulator which is specially valuable in its application to industries. In jam factories, for instance, the girls often badly scald their hands owing to the water used for washing jars becoming too hot. To take another case, the hot potash used by electro-platers can be kept at the most efficient temperature and prevented from boiling over. The thermostat, which does the regulating, is dependent upon the fact that a tube of brass expands with heat while a rod of nickel steel within does not appreciably change its length.

APPOINTMENT.—We are pleased to notice that Messrs. E. Dent and Company, Limited, of 61, Strand, and 4, Royal Exchange, have received a royal warrant, appointing them watch, clock, and chronometer makers to His Majesty King George the Fifth.

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QUERIES AND ANSWERS.

Readers are invited to send in Questions and to answer the Queries which are printed on this page.

QUESTIONS.

Numbers 14, 16, 17, 18, and 20 (Page 461) still remain unanswered.

21. ASTRONOMY AND GEOGRAPHY.—Will any fellow-teacher advise me as to good school experimental and observational courses in astronomy and geography?

BELLERBY LOWERISON.

22. RADIUM.—Seeing that Radium is not a stable substance, but is continually changing with other forms of matter, none of which are stable as far as we know, how is it that Radium is still found in the crust of the earth?

J. G. SPARKE (Lieut.-Colonel).

23. THE GULF STREAM.—A friend mentioned the Gulf Stream the other day, and I remarked that I understood that this was now considered to have no effect whatever on Britain. Will you briefly inform me, in "KNOWLEDGE," if I am correct, and what are the facts of the case; how far away is the nearest trace of this stream perceptible, and so on?

S. P. ROWLANDS.

24. DREAMS.—I should be pleased if some of your readers would give an explanation of the following common occurrence in dreams. Suppose one dreams of an impending railway collision. In the ordinary sequence of events, one dreams of the two trains approaching one another before the actual collision. Now it is a peculiar fact the collision is frequently emphasised in the world of reality by a door banging, or some such sudden noise. In fact, one might almost say that every real occurrence impressed on our senses during sleep fits in perfectly with the whole plan of the dream. In the example I have just cited one expects a noise at the moment of the collision, and it happens. If that particular door was not about to slam it is probable, in fact almost certain, that that dream would not have occurred.

Do you think this any evidence that the mind is often aware of the events of the future?

J. C.-W.

TIDES.—There is a point in connection with the tides of the ocean which I have never seen elucidated in any popular manner.

Tidal friction causes retardation in the rotation of the earth, and by the principle of conservation of the moment of momentum the revolution of the moon is accelerated. This is the basis of Darwin's theories of tidal evolution, and, of course, remains true whatever may be the lag of the tides behind the moon. The acceleration of the moon is sometimes further explained by a diagram illustrating that the nearer tidal protuberance exerts greater attraction on the moon than the one opposite, much as the precession of the equinoxes is always illustrated. Now the depth of our oceans being less than the critical depth of about fourteen miles, our tides are inverted, that is, high tide where low tide would be expected, and *vice-versa*; but if the depth were greater than fourteen miles the tides would be direct, and the nearer tidal protuberance should be behind instead of in front of the moon, and it seems to need popular explanation why a retardation of the lunar revolution does not result. The whole question of the lag of the tides on an ocean-covered earth might receive more notice in popular accounts, though probably too mathematical for full elucidation.

J. H. G.

REPLIES.

10. WATER AND ITS OWN LEVEL.—I am a little perplexed myself about what G. G. B. has mentioned about water and its own level.

It is, I believe, one of the strongest arguments of flat earthists, and the flat earth idea is certainly wrong, so there must be some explanation that will answer his query.

Is it that, taking for example a square mile of ocean, it is such a small area that the water certainly finds its own level within that radius, but taking the oceans themselves they must take the curvature of the earth, so in the one sense does the water find its own level, while in the wider sense we must also forget that the earth has a great power of attraction, and so the gravitation of the earth makes the oceans take the shape of the globe.

A. MERCER.

11. A BOOK ON WASPS.—In answer to Mr. Sandeman's enquiry for a book on wasps, I venture to recommend "Wild Bees; Wasps and Ants and other Stinging Insects," by Edward Saunders, F.R.S. It is a popular work of about one hundred and fifty pages, with four coloured plates, and other illustrations. published two years ago by Routledge. The price, I think, is 3s. 6d.

M. A. S.

13. THE FINDING OF THE TIME AT NIGHT.—The time may be found at night approximately by means of the accompanying adjustable dial; this may be used either by direct comparison, or studied until it can be retained in the memory sufficiently for comparison with observation.

Polaris and β Ursae minoris.					
1 Jan.	1 July	VIII	1 Apr.	1 Oct.	II
1 Feb.	1 Aug.	VI	1 May	1 Nov.	XII
1 Mar.	1 Sept.	IIII	1 Jun.	1 Dec.	X

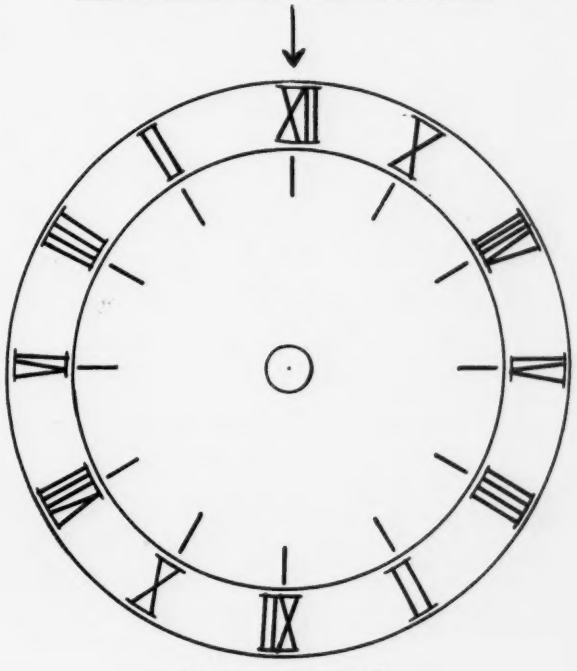

FIGURE 1. The Dial.

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The moveable circle is adjusted in accordance with the little table. The number given for the 1st of the month is brought opposite the pointer, which represents the top of the imaginary celestial dial at the Pole. The circle is then moved on, counter clockwise, through the fraction of the next two-hour interval which corresponds to the fraction of the month elapsed.

In direct comparison the dial is held up with its back towards the Pole, with the pointer at the top. The line joining Polaris and β Ursae Minoris is compared with it; and an imaginary line is drawn across the centre of the dial parallel to that line. This imaginary line shows the time according to the hours marked on the dial.*

If it is desired to study the method so as to be able to work without the dial, the student may keep the dial by him, adjusting it every few days. He should study the dial, the adjustment of which changes slowly, and remember it, so as to be able to work without it at night; using it, if necessary, for verification. The changes run through a year, sometimes the one star and sometimes the other being uppermost. After a year's practice the changes should be sufficiently engraved on the memory to admit of finding the time roughly without the direct use of the dial. But the scheme is a little complicated; and it will be convenient to have the dial at hand to refresh the memory.

The principle is not difficult. All star phenomena occur about four minutes earlier for every day that passes. This arises from there being one more sidereal day in the year than the number of solar days. The resulting change of twenty-four hours is distributed over the year in a manner strictly proportional to mean time. Thus, if the months were of exactly equal length, the change in each month would be exactly two hours. As it is, a small irregularity arises from the varying lengths of the months, which may be neglected for our purpose.

It is desirable to take stars always visible. We take the Pole-star and β Ursae Minoris. The latter is the nearer to the Pole of the two conspicuous stars β and γ Ursae Minoris, which lie near together at distances of about sixteen degrees and eighteen degrees from the Pole respectively.

The times indicated by the line joining any two stars near the Pole can be determined by a single accurate observation. This observation should be made when the line is either vertical or horizontal, or preferably, observation should be made in both positions. In other positions the observation is much less certain. In the present case the observations were made a day or two before and after 1st December, 1910, when the vertical position occurred at ten o'clock, and the horizontal at four. This determines the table of adjustments.

In estimating angles it is sometimes of use to note that the stars β and γ Ursae Minoris subtend just half an hour at the Pole; and we may take it that the angle at Polaris is sensibly the same.

In one of Hardy's novels (I think "Far from the Madding Crowd") the shepherd is said to be able to tell the time from the stars. I have often speculated whether this is possible for an uneducated man. It appears to be possible, though improbable, given the outdoor habit at night, a clear head, and either a good memory, or a habit of making notes.

Any observant person much out at night must notice that the star phenomena change their times, being earlier every night. Also he might notice that they recur after a year. The deduction of the two hours change per month seems to present the greatest difficulty from this point of view, though it is quite simple when explained. Then, one good observation only is required to determine the time by any line joining two stars near the Pole. But it would want a clear head to keep the changing scheme of the hours always in view.

The same moveable disc would do for any pair of stars near the Pole, the setting table only being determined by observation.

This method is not susceptible of considerable accuracy. It is a recognised principle in naked eye astronomy that the roughest measurement is better than the best eye estimate. Here there also appears to come into play, for positions intermediate between the vertical and horizontal, one of those illusions which depend on the structure of the eye. I myself see the bisector according to time between the horizontal and vertical positions decidedly nearer the vertical than the horizontal. This is in the sky, not in the dial, which subtends a comparatively small angle at the eye. The resulting error may be either fast or slow, according to the position. I believe it depends on the fact that, in a figure subtending a considerable angle at the eye, the eye estimates vertical distances on a larger scale than horizontal ones. This may arise from the retina being slightly flatter vertically than horizontally. If one is able to estimate the amount of this error for one's own eye, one may allow for it roughly. The vertical and horizontal positions admit of fairly accurate observation.

R. H. M. B.

ECLIPSES.—The two following questions have not been numbered, as they are here answered.

Does a total eclipse of the sun occur, on an average, ten times in one hundred years—of course, astronomically?

A total eclipse of the sun may occur on the earth more than once in a year. From the year 1851 forty total eclipses have occurred, but may have not been observed (astronomically) owing to inaccessibility (near the poles), to being only visible at sea, or to cloud interference. Total eclipses of the sun occur in cycles, the path of total phase gradually moving southward or northward over the earth's surface and taking over one hundred years to return to a similar point; and a cycle of about fifty-four years, which may be divided into three periods of eighteen years, the area of total phase moving east to west; so that every fifty-four years the eclipse is at the same longitude, though gradually moving to greater or less latitude, according to the other cycle movement.

Was a total eclipse of the sun visible at Greenwich in the year 1900?

No total phase was visible at Greenwich in 1900; no total eclipse had been visible in Great Britain for about one hundred-and-fifty years; there will be a momentary one about 1927, and again in 1999, and possibly for a second or so in one or two other years this century.

NOTICES.

X-RAYS.—The Sanitas Electrical Company, of 61 New Cavendish Street, W., have sent us a booklet containing a large number of testimonials from workers in X-rays and electro-therapeutics, as well as physicists, upon the results obtained with their apparatus, including "Sanax" X-ray Outfits, Intensified Induction Coils, and the Motor Mercury Interrupter; as well as their "Multostat" Universal Apparatus. In addition to illustrations of the apparatus, reproductions of two remarkably fine radiographs are given.

EXPOSURE RECORD.—"The Wellcome Photographic Exposure Record and Diary for 1911" has also reached us. It contains a great many hints for the benefit of photographers, in addition to a neatly arranged series of tables in which the owner may record all the details with regard to his exposures. There is, besides a diary, an exposure calculator and several interesting illustrations, including the reproduction of a colour photograph developed with tabloid photographic chemicals. The price is one shilling.

* NOTE.—The motion round the Pole looking N. appears counter clockwise. Looking S. the motions seen appear clockwise.

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